Empirical Studies of Consumer Search and Market Power

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Abstract

This thesis explores the interplay of search frictions and market power. In the first essay, we study how prices are negotiated between consumers and firms. In the electricity market that we study, with competitive retailers, fixed and variable charges vary widely across consumers. We implement an audit study to identify the sources of price dispersion. We create a call centre staffed by actors that call real call centres to obtain rates for fictitious consumers with experimentally-assigned combinations of consumer characteristics. We find that offline search leads to larger discounts than online search. Firms reduce their profit margins by 30% for call-in consumers who are informed about and who negotiate using low reference prices. We also document cross-sectional price discrimination between new consumers in a market and existing consumers. Holding price informedness and other consumer characteristics fixed, firms are less willing to negotiate lower prices with new arrivals than with existing clients of rival firms.

My second essay investigates the impact of a mandatory information disclosure policy on market competition in the retail gasoline context. Information disclosure policies enhance search and are implemented with the aims of increasing demand elasticities and creating competition. However, if price transparency also makes it easier for firms to monitor their rivals' behaviour, this raises concerns about tacit collusion. As such, the equilibrium impact on competition depends on which effect dominates. My study shows that the price disclosure policy leads to margin-enhancing effects in small regional markets. Digging deeper, I find that these margin-enhancing effects are directly associated with an equilibrium price transition, where a dominant firm uses price leadership to communicate their intention to transit from a price cycle equilibrium to a more profitable fixed price equilibrium. This transition, which occurred immediately after the information disclosure policy was introduced, suggests that firms were potentially using the platform to coordinate with each other.

The final essay investigates how consumer search on price transparency platforms varies across socio-economic groups. In recent years, there has been a push for demand-side policies that aim to help consumers, especially disadvantaged households, make more informed decisions. However, it is not well-known who these initiatives benefit most. Therefore, this essay investigates how users on a price transparency platform who belong to different

socio-economic backgrounds respond to changes in price dispersion. In the context of retail gasoline, my analysis reveals heterogeneous search responses to changes in price dispersion across socio-economic groups. In particular, I find that users who get the most value from search relative to income, such as the most vulnerable households, are least engaged in search.

Declaration

This is to certify that:

1. the dissertation comprises only my original work towards the PhD except where indicated in the Preface,
2. due acknowledgement has been made in the text to all other material used,
3. the dissertation is fewer than 100,000 words in length, exclusive of tables, maps, bibliographies and appendices.

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Preface

This thesis contains original research in Chapters 2 through 4.

Chapter 2 is based on the following working paper:

Byrne, D.P., Martin, L.A. and Nah, J. (2019). Price Discrimination, Search, and Negotiation: A Field Experiment in Retail Electricity.

Chapter 3 is based on the following working paper:

Nah, J. (2019). Information Disclosure and Price Coordination in Retail Gasoline Markets

Chapter 4 is based on the following working paper:

Nah, J. (2019). Retail Search and Socio-economic Disadvantage in Retail Gasoline Markets.

All co-authorship has taken place in accordance with the Graduate Research Training Policy of the University of Melbourne.

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Last but not least, I would like to thank my husband, to whom I dedicate this PhD thesis. Your love and support over these five and a half years have been pivotal to the completion of this thesis. I love you so much.

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Chapter 1

Introduction

In homogeneous goods markets, standard economic models predict that competition among firms will lead to a single price. However, many empirical studies have shown evidence of price dispersion even when goods are identical. Some economists argue that subtle differences between firms could lead to price dispersion but this claim was refuted by Stigler (1961), who suggests that while a portion of price dispersion may be attributed to firm differences, it would be impractical to declare that all dispersion is due to heterogeneity. Since then, a large literature has been dedicated to studying price dispersion and there have been many theoretical (Janssen and Moraga-González, 2004; Salop and Stiglitz, 1977; Stahl, 1989; Varian, 1980) and empirical (Allen et al., 2014; De los Santos et al., 2012; Honka, 2014; Wildenbeest, 2011) evidences of search frictions explaining most of the dispersion observed.[1]

Isolating the effects of search and price dispersion is empirically challenging because they both coexist in equilibrium. This thesis overcomes this issue by using the following empirical techniques. In the first essay, we run a randomised controlled trial, which allows us to have control over the conditions that influence firms' pricing decisions, thus enabling us to disentangle the various factors that affect price dispersion. The second essay exploits a natural experiment from which I am able to form causal inferences of the effects of a

[1] See Baye et al. (2006) for an overview of the literature.

search technology on market competition. Furthermore, with access to real-time firm-level data, I am able to directly observe how firms interact with each other, which is important for studying tacit collusion. Finally, in the third essay, I use data from an online platform to study how consumers search. The identification strategy here relies on the low adoption rate of this platform at the time of this study, which implies that prices are likely to be exogenous to search on the platform. In addition, I use direct measures of search and price dispersion in my estimations. This contrasts with previous studies that have relied on indirect search methods, such as frequency of purchase (Sorensen, 2000), internet adoption (Brown and Goolsbee, 2002) and spatial distance between firms (Chandra and Tappata, 2011) to estimate search costs.

This thesis contributes to the understanding of how search frictions influence firms' pricing decisions, how consumers search and how certain policies that help consumers search may have unintended consequences.

In many markets in which firms have market power, prices are privately negotiated between consumers and firms. Therefore, in Chapter 2, we study how search frictions affect negotiated prices. In the retail electricity market that we study, there are many competitive retailers. Yet, prices vary widely across consumers. Since there are numerous factors that can affect individual prices, we implement an audit study to identify the sources of price dispersion in this market. In particular, we created a call centre staffed by actors that call real call centres to obtain prices for fictitious consumers with experimentally-assigned combinations of consumer characteristics. We find that offline search by calling in leads to larger discounts than online search. Firms are willing to reduce their profit margins by 30% for consumers who reveal a low reference price from a rival firm. Holding price informedness and other characteristics fixed, we find that consumers who reveal that they are switching from a rival firm obtain larger discounts than consumers who are new in the market. In addition to showing how search frictions lead to profit-enhancing outcomes, we also contribute to the literature by bringing the audit study approach to IO for studying price discrimination, search frictions and negotiation as sources of price dispersion and market power.

In response to empirical evidences that show search frictions as a source of market power, many competition authorities and industries are considering mandatory price disclosure policies. However, there is currently little evidence on the effects of such price transparency regulations on prices and margins. Do these regulations result in more or less competitive markets? While the motivation behind these policies is to lower search costs and create competition, the availability of price information can also be detrimental to competition if firms use these information to coordinate with their rivals. The equilibrium effect would then depend on which of these effects dominates.

Chapter 3 studies the impact of a mandatory information disclosure policy in a retail gasoline industry. This policy that requires all gas stations to update prices on an online platform in real-time provides users perfect knowledge of prices in the market. The availability of monthly prices from a large cross-section of markets in both pre- and post-intervention periods enables me to investigate the causal effects of the policy on margins. I find evidence of margin-enhancing effects in small regional markets after the policy was introduced. Digging deeper, I analyse real-time station-level prices to investigate how gas stations interact with each other under this policy. My analysis reveals a state-wide coordinated transition to a more profitable pricing equilibrium in regional markets led by one of the big players, which suggests that firms were potentially engaging in tacit coordination under the policy.

Despite concerns of softer competition, price transparency policies are still popular in many jurisdictions and in some cases, they can be welfare-enhancing if consumers are able to engage in effective search. The issue is, search can often be challenging. For instance, many retail gasoline markets exhibit confusing and unpredictable price cycles (de Roos and Smirnov, 2017), which makes it difficult to predict the best time to search. The concern for policy is that consumers or households who can gain the most from searching on these platforms, such as vulnerable and low-income households, may be the ones finding it difficult to time their search in order to take advantage of the lowest prices.

Against this backdrop, my essay in Chapter 4 explores how consumers search in response to supply-side price shocks and how this response varies across socio-economic groups. For this analysis, I use auxiliary search data and price data from the price transparency website studied in Chapter 3. I find that users in the most disadvantaged socio-economic group respond the least to changes in price dispersion in the market compared to users in higher socio-economic groups. My results reveal that those who get more value from search relative to income are less engaged in search and underscore the importance of policy improvements that simplify search for users who fail to engage in relatively challenging intertemporal price search.

The remainder of this thesis is structured as follows. Chapters 2 to 4 form the core of this thesis, while Chapter 5 concludes. Appendices A to C contain the supplementary material corresponding to each of the core chapters.

Chapter 2

Price Discrimination, Search, and Negotiation: A Field Experiment in Retail Electricity

2.1 Introduction

In many important markets in which firms have market power, prices are negotiated between consumers and firms. Examples include banking, healthcare, telecommunications, energy, private schools, and retirement communities. In these markets, it is not uncommon for firms to publicly post prices and privately negotiate discounts with consumers. In these markets search frictions and negotiation are key determinants of retail price dispersion. If firms price discriminate based on willingness and ability to search and negotiate, the distributional welfare impacts of market power depend on the willingness and ability of different consumers groups to engage in these activities.

Despite its prevalence, there is surprisingly little research into price posting, negotiation, and price discrimination in search markets. This is partly due to the fact that firm-consumer specific negotiated price data are sensitive and generally unavailable. Early research on price dispersion and market power (e.g., Sorensen (2000) and Brown and Goolsbee (2002)) focuses on dispersion in posted prices and abstracts from negotiated prices. Recently, Allen et al. (2018) gain access to consumer-specific mortgage contract data from Canada and provide a structural analysis of search frictions, negotiation, and branding as sources of price dispersion and market power in retail banking. Hastings et al. (2017) similarly exploit administrative data to provide a structural analysis of search, branding, and market power in the context of Mexico's privatisation of social security.

While these studies provide new frameworks for the analysis of market power and its sources, their applicability is limited to the extent that researchers face significant barriers to accessing firm-consumer specific negotiated price data.[1] In this essay, we propose a field experimental approach to recovering such negotiated price data. It is based on audit studies, which have an extensive history for studying labour market discrimination (Bertrand and Duflo, 2017). Our innovation is to bring the audit study approach to IO for studying price discrimination, search frictions, and negotiation as sources of price dispersion and market power. In this way, we emphasise audit studies as an empirical tool for studying the oligopoly problem more generally.

Our audit study approach complements previous structural analyses in two important ways. We explicitly design our field experiment to disentangle different mechanisms that affect price negotiations. In our study, we primarily focus on search frictions as a key mechanism for determining negotiated price outcomes between firms and consumers. Moreover, the simplicity of our approach makes it readily accessible to policymakers for more accurately measuring price dispersion and market power, and identifying their sources.

We conduct our experiment in a competitive retail electricity market.[2] This context is well-suited for several reasons. First, the product is homogeneous, which allows us to abstract

[1] Relatedly, Backus et al. (2018) examine bilateral bargaining over prices using data on sequences of price offers between buyers and sellers on eBay. However, they abstract from market power and interdependencies in bargaining processes across oligopolistic sellers, which is a focus of our study.

[2] In many countries, including the US, UK, Australia and across Europe, there is retail competition and price dispersion in electricity markets (Hortaçsu et al., 2017).

from product differentiation in identifying the role of search frictions in creating retail price dispersion (Koulayev, 2014; Wildenbeest, 2011). Second, retail electricity markets are local, and the entire population of firms offering posted and negotiated prices is identifiable. We can therefore recover the entire distribution of posted and negotiated prices across firms.[3] Finally, there are no explicit switching costs from changing retailers, which allows us to avoid complications associated with disentangling search frictions and switching costs (Handel, 2013).

Electricity is also a highly policy-relevant context. It is an essential service whose use creates pollution externalities, for which there is prior evidence of retail market power (Guilietti et al., 2014; Hortaçsu et al., 2017). Indeed, in the specific market we study of Victoria, Australia, there are concurrent state and national inquiries into market power (ACCC, 2018c; Thwaites et al., 2017). These inquiries respond to questions about whether retail competition in electricity should be abandoned in favour of regulated monopoly. Our study informs these debates over market design by quantifying the degree to which price negotiation in competitive retail markets dissipates monopoly rents, which has previously gone unmeasured.

Finally, our experiment is partly motivated by consumer advocacy concerns about incomplete pass-through of government subsidies to low-income consumers. Electricity represents a significant portion of expenditures for low-income consumers and high bills can exacerbate cycles of debt and poverty (Johnston, 2016). A number of governments worldwide therefore subsidise rates for low-income consumers. Examples include social tariffs in France, Low Income Home Energy Assistance Program (LIHEAP) and linked eligibility utility-sponsored

[3] In contrast, previous audit studies on price negotiation, where the focus is on how reference prices for negotiation and gender influence bargaining outcomes, involve competitive markets where data on the entire distribution of posted and negotiated prices are not available. See, for example, Busse et al. (2017) (car repairs), Castillo et al. (2013) (taxis), Gneezy et al. (2012) (wheelchairs, new cars), List (2004) (sports cards), and Ayres and Siegelman (1995) (used cars). Also, we focus on a market that entails regular quarterly transactions (electricity bills), whereas previous studies study contexts with more infrequent transactions for durable goods or collectables.

programs in the US like the National Grid's Energy Affordability Program, and the Warm Home Discount in Great Britain (The Brattle Group, 2018).

There is a large literature in economics on the extent to which the benefits of subsidies pass-through to consumers, including in markets with imperfect competition (Weyl and Fabinger, 2013).[4] When subsidies are targeted ("tagged") and suppliers have market power, perhaps due to search costs, it can be profitable to charge subsidy recipients higher base rates (Akerlof, 1978). There is recent empirical evidence of this practice. Collinson and Ganong (2015) show that a dollar increase in the price ceiling of Housing Choice Vouchers led landlords to raise tenant rents by 13 to 20 cents and Turner (2017) shows that universities reduce their individual aid packages by 19 cents for every dollar of federal need-based Pell Grant that a student receives. In each of these cases, supplier responses undermine the effectiveness of the subsidy. Muehlegger and Rapson (2018) nonetheless show that incomplete subsidy pass-through is not inevitable: a program to subsidise the purchase of hybrid electric and electric vehicles among low-income households in California, that involved extensive monitoring of sale prices by regulators and screening of eligible suppliers, did not lead to price discrimination based on subsidy status.

Finally, the ACCC report uses highly-sensitive retailer data to show that concession consumers in Australia pay higher base rates than non-concession consumers (ACCC, 2018c). The difference in base rates in Victoria translates to 24% of the concession subsidy paid by state government being captured by retailers in the form of higher prices.

[4] Typically, product-based purchase subsidies are available to all local purchasers of the product. For example, Lade and Bushnell (2016) find that only half to three-quarters of the subsidy for the purchase of ethanol-based fuels is passed-through to consumers. Rodgers (2018) finds that 50 cents of every dollar of the US Child and Dependent Care Credit is captured by providers in the form of higher prices and wages. Cabral et al. (2018) find evidence of incomplete pass-through of government subsidies to private Medicare Advantage plans, with substantially higher pass-through rates at 74% in the most competitive market as opposed to 13% in the least competitive market. This result echoes the heterogeneous tax incidence of fuel prices found by Stolper (2016) when comparing retail gasoline markets with different levels of local competition. There is also empirical evidence of incomplete pass-through of purchase subsidies in individually-negotiated prices. Busse et al. (2006) find that auto dealers increase negotiated rates not only when consumers benefit from dealer cash promotions but also under consumer cash promotions. Gulati et al. (2017) find that dealer margins rise by $138 for every $1,000 increase in the subsidy for hybrid electric vehicles in Canada.

Observing higher prices for subsidy recipients in aggregate data does not necessarily reflect questionable pricing. Subsidy recipients could be more costly to serve if, for example, they consistently use less power, or are more likely to default on their bills. They could also be less likely to search. Gulati et al. (2017) provide a simple theoretical framework that explains why, when bargaining is costly to consumers, subsidy-status may in itself lower the amount of search and negotiation that takes place.

Our experiment is set up in a way that allows us to determine whether recipients of electricity purchase subsidies are being explicitly targeted with higher base rates. By exogenously varying each customer characteristic independently, and collecting prices for combinations of characteristics that may be infrequently observed in practice, our experimental design allows us to disambiguate the subsidy status from cost of service and willingness to search. To the extent that price dispersion is driven by search costs, there are low-cost strategies available to governments to reduce search costs that could yield efficiencies in the design of electricity concession payments[5].

Our field experiment is structured as an audit study that obtains price quotes from electricity companies for fictitious customers with randomly-allocated combinations of characteristics. Our experiment sees actors engage in scripted phone conversations with electricity retailer call centre personnel, revealing over the course of each conversation randomly-assigned consumer characteristics and informedness of competitors' prices.

These experimental phone calls yield a dataset of negotiated retail prices that we match to online posted prices from the retailers' websites. Combining the posted and negotiated price data, we are able to estimate the extent to ignorance over rivals' prices explain variation in posted and negotiated retail prices.

Our experiment delivers a number of new insights into the interconnected impacts of price discrimination, search, and negotiation on retail prices. Quantitatively, we document a

[5]Concession payments are government subsidies for electricity costs that are offered to a subset of households. Consumers qualify for such payments by having low incomes, being a pensioner with a moderate to low income, or being a veteran.

substantial, 30% reduction in profit margins, from 8 to 11 percent mark-ups over costs, if consumers threaten to switch retailers and engage in price negotiation using low, reference prices.[6] These baseline results are revealing of a market with discriminatory pricing whereby firms post high prices that are paid by disengaged consumers, while consumers engaged in price negotiation realise large price discounts. Being able to observe the entire distribution of posted and negotiated prices from our experiment is fundamental to obtaining these results.

We further document two novel empirical results for empirical research on retail price search.[7] First, we experimentally vary whether a consumer is new to the market, or is an existing consumer looking to switch retailers. We find firms offer significantly higher prices to new consumers and are far less willing to negotiate with them. In other words, we find that perceived consumer experience in the market is an important factor that firms condition when engaging in price discrimination.

Second, we exploit the fact that we have an identifiable finite number of firms in our market and experimentally vary where in a sequential price search process consumers are when negotiating prices. With a strong caveat on statistical significance, we find some evidence that firms' prices depend on where consumers are in a sequential search process.

Finally, conditional on likelihood to search and willingness to accept direct-debit or pay-on-time plans, we find no evidence of price discrimination based on a consumer's government subsidy status. Incomplete pass-through of government subsidies for vulnerable customers appears to be due to lower likelihood of search and lower willingness to accept direct-debit or pay-on-time plans.

This chapter is structured as follows. In Sections 4.2 and 2.3 we describe the industry and our experiment. Section 3.5 presents our results, and we conclude in Section 4.6.

[6] As we discuss in detail below, we exploit recently-published in-depth inquiries from multiple government agencies into market power in the retail electricity market. These reports exploit proprietary consumer-firm specific pricing data, as well as firms' consumer-specific cost data, to provide estimates of average mark-ups for the entire market, as well as for different sub-groups of consumers. We make use of these figures to provide relevant context for our experimental findings.

[7] See (Baye et al., 2006) or (Ellison, 2016) for overviews of the empirical literature on retail price search and price dispersion.

2.2 Industry

Our research context is the electricity market of the Australian state of Victoria.[8] The market is split into four parts: generation, transmission, distribution, and retail. Generators compete every 5-minutes in uniform price auctions that determine the marginal wholesale cost of generating electricity. Distributors are regulated monopolists who own the electricity grids, the wires and poles, and manage geographically-distinct electricity transmission and distribution networks. Competing retailers pay network fees upstream to buy electricity from distributors. They in turn supply electricity downstream to end users, both residential and commercial.

In the retail market, there are 17 firms during our 2016 study period: 3 big, 3 medium, and 11 small.[9] These groups of retailers respectively have market shares of 60%, 28% and 12%. The "Big 3" retailers, AGL, Origin, and Energy Australia, are vertically integrated and compete in both the generation and retail markets. This market structure is relatively mature as retail competition was introduced in 2009. Prior to then, retail electricity prices were regulated by the state government.

2.2.1 Retail pricing

As in many electricity markets, retail electricity prices in Victoria typically consist of a two-part tariff: a fixed daily charge irrespective of electricity used, and a variable per kWh charge. At an average of AUD $1/day fixed and 27 cents/kWh variable, prices in Victoria are slightly higher than those typically offered in the United States, and lower than those available

[8]According to the 2016 Census, Victoria has a population of 6.3 million people, 4.4 million of which live in the state capital of Melbourne.

[9]All figures referenced in the discussion institutional detail that follows in Sections 2.2.1 and 2.2.2 are drawn from four major industry reports into the retail market from the Australian Competition and Consumer Commission (ACCC, 2018c), Australian Energy Regulator (AER, 2017), Australian Energy Market Commission (AEMC, 2017), and a state-level retail electricity market review by the Victorian Government (Thwaites et al., 2017). Data on prices, costs and margins are drawn from either ACCC (2018c) or AEMC (2017), who both have access to highly proprietary detailed firm-consumer specific data on contracts and costs of service from all firms in the market for their investigations into retail electricity markets.

in Europe. Some retail pricing contracts involve increasing block tariffs, flat variable charges, and time-of-use variable charges by time-of-day and day-of-week. Our experimental design abstracts from increasing block tariffs by focusing on average energy usage levels. We focus on the prices most commonly offered in the market: contracts with flat variable charges.

Consumer rates can be categorised into three sets of prices: *default contracts*, *posted prices*, and *negotiated prices*.[10] If customers never adopt a posted price contract, or fail to renegotiate a posted price contract after it expires, they are switched to a default contract. Some posted price contracts never expire; others last one or two years. The government requires that every retailer offers default contracts in order to ensure that customers always have a valid contract irrespective of their level of engagement as a shopper in the retail market.

Posted prices are more competitive than default contracts. Customers can obtain these contracts by signing up online or calling their current retailer or competitor. Posted prices are often expressed as a discount relative to that retailer's current default contract. A retailer typically offers multiple posted prices at any given time, with variation in the ratio of fixed to variable charges, discounts for direct debit or on time payments, green power commitments, or one-time sign-up discounts or other promotions.

Finally there are negotiated prices. Some consumers are on contracts that are negotiated by trade associations for the benefit of their members. Others are on contracts negotiated directly by consumers, either when contacted by rival retailers or third-party resellers, or when consumers initiate contact by calling retailer call centres. Prior to this essay, there was only anecdotal evidence to the potential gains from calling up and negotiating rates in this market. Online price comparison tools only compile data about default contracts and posted prices. Aggregate industry statistics either do not account for negotiated discounts at all, or do not distinguish between posted price contracts obtained at different times and negotiated

[10] In Victoria, default contracts are referred to as "standing offers" whereas posted prices and negotiated prices are called "market offers".

price contracts. This is, in part, the measurement problem our field experiment below helps to resolve.

The state government also subsidises electricity costs for a subset of households through concession payments. Consumers qualify for such payments by having low incomes, being a pensioner with a moderate to low income, or being a veteran.[11] Eligible consumers are required to contact and provide their concession card details to their retailer in order to benefit from concession rebates. The annual Victorian concession is set at 17.5 per cent of electricity usage and service costs after retailer discounts and solar credits have been applied. The concession does not apply to the first \$171.60 of the annual bill. Concession rebates are calculated by retailers and deducted directly from the total nominal costs on each bill. Consumers observe the nominal cost, concession amount and the net payable amount.

2.2.2 Demand

Victorian households consume an average of 4800 kWh of energy per year. This costs them \$1457 in electricity bills annually on average, representing about 3% of total disposable income.[12] Among the bottom 20% of income earners, electricity bills represent a significantly larger portion – approximately 10% – of income, which is in part why the state government provides concession payments to these groups.

Apart from electricity consumption, consumer search and retailer switching is a key aspect of demand. Each year, 26% of consumers switch retailers. There is, however, considerable inertia with electricity contracts, which has in part led to an incumbency advantage for the "Big 3" retailers, as evidenced by their large market shares. Using proprietary data from all retailers' customer account databases, which include consumer prices, costs, and turnover,

[11]Specifically, an individual who resides in Victoria, Australia is eligible to apply for annual electricity concession if they own one of the following cards: Pensioner Concession Card, Health Care Card or Veterans' Affairs Gold Card.

[12]This compares, for example, to 16% and 18% of disposable income on average being spent on food and housing, respectively.

ACCC (2018c) documents that among the Big 3, 30% of consumers on posted price contracts have been with their retailer for more than 2 years, while 75% of consumers on default contracts had not switched retailers in more than 2 years. Among all other retailers these figures are just 18% and 20%, respectively, highlighting a much lower degree of inertia for the mid-sized and small retailers.

Retailers attempt to overcome this inertia by engaging in door-to-door selling and telemarketing, as well as online and cable advertising, all of which encourage customers to switch from their current retailer.[13] Leveraging retailers' internal cost data, ACCC (2018c) estimates that 8% of a consumers' total bill typically is spent on consumer billing, marketing, and assistance costs. Moreover, it has been well-documented that the combination of relatively complex electricity pricing contracts and constant marketing campaigns leave consumers generally confused and creates large search costs that limits consumer engagement.[14] To help combat this, the state and national government both offer online price comparator websites to help customers compare electricity pricing contracts in making switching decisions.[15]

What fraction of customers end up on higher-priced default contracts and lower-priced posted contracts as a result of this switching behaviour? Again leveraging proprietary data from the retailers, ACCC (2018c) reveals that 6% of customers in the state end up on default contracts. Hardship consumers are twice as likely to be on default contracts, compared to non-hardship consumers.

[13]Marketing and consumer switching intensity is most intense around January and July each year, as this is when upstream electricity distributors update their network charges, and retailers update their prices.

[14]See ACCC (2018c), AEMC (2017) and Thwaites et al. (2017). This issue of consumer inertia in retailer choice in retail electricity markets is not unique to our setting. Guilietti et al. (2014) and Hortaçsu et al. (2017) similarly document significant search frictions and inertia in U.K. and U.S. retail electricity markets.

[15]Energy Made Easy is the national website (https://www.energymadeeasy.gov.au/) while Victorian Energy Compare is the state-run website (https://compare.energy.vic.gov.au/).

2.2.3 Margins

Lacking data on negotiated prices and firms' costs of supply has, historically, made it difficult to estimate retail margins in the market. However, through its unique access to consumer-level contract data and firm cost data, ACCC (2018c) estimates that Victorian retailers earn an 11% profit margin on average.[16] In dollar terms, this implies that $160 of a customer's $1457 annual before-tax electricity bill is retail profit. Moreover, historical data obtained by the ACCC reveals that these nominal per-consumer annual margins have fallen by just $4 (in 2015-16 dollars) since 2007-08, the year before the retail market was deregulated. Market power has persisted in the industry over time despite the introduction of retail competition. Indeed, between 2007 and 2016, annual per-consumer profits in real terms have risen from $123 to $163, which represents a 33% increase.

2.2.4 Summary

Summarising our discussion of institutional detail, the market is a homogeneous product market with asymmetric retailers and three dominant firms. There is significant inertia among consumers in retailer choice, and they face potentially confusing non-linear two-part tariffs when searching for lower prices. Such deals exist if consumers are willing to search and negotiate, as industry reports based on proprietary data are revealing of significant retail price dispersion. This dispersion arises as firms offer posted price contracts whereby they discount variable per-kWh prices relative to the variable per-kWh prices in default contracts. These latter contracts, in effect, serve as an upper bound on retail prices. The main policy issues in the market are twofold: (1) rising margins over time since the market was deregulated in 2009; and (2) low-income consumers paying higher prices, potentially as a result of not being engaged in retail price search and negotiation.

[16] As described in the ACCC report, all margin figures correspond to earnings before interest, taxes, depreciation and amortisation (EBITDA).

2.3 The Experiment

In this section, we describe a field experiment designed to quantify the degree of price dispersion and its underlying sources. The experiment generates a publicly-available dataset that acts as counterpart to the highly proprietary administrative data on consumer-firm specific contracts used by ACCC (2018c) for its antitrust investigation. We first describe our experiment, which is an audit study whereby fictitious electricity customers under different experimental conditions call retailers to negotiate prices. Having described the experiment, we describe our dataset which consists of experimental price data and retail electricity contract data that we scraped from electricity retailers' websites.

2.3.1 Design

Our fictitious electricity customers were actors who we recruited from an online acting recruitment website in Melbourne. We held a casting call at the University of Melbourne where we interviewed actors using hypothetical bargaining scripts. In total, we hired 18 different actors for the experiment, 9 of which were female.

All successful recruits participated in a four-hour training session where we informed them about the study and the structure of the retail electricity market in Victoria. We also had the actors practise negotiating electricity contracts with each other whereby one acted as the electricity retailer and the other was the customer. We finished training by having actors engage in pilot negotiations with actual electricity retailers using different bargaining scripts.[17] In practice, negotiating retail prices with retailers amounted to our actors calling front-line employees at retailers' call centres who were the first point of contact for consumers.

We developed 28 fictitious customers. Each customer is a combination of one of 4 characteristics: new arrival vs. client of rival firm, subsidy-recipient or not, reference price

[17] The bargaining scripts used in our experiment are provided in Appendix A.1.

(high or low), and source of reference price (called 1, called 3, price comparator website, friend). We span the entire space of possible combinations, using two different reference prices for all but the easily-verifiable price comparator website.

We called every retailer with every consumer combination. We randomly assigned actors to the 28 by 12[18] = 336 customer-retailer treatments. Our intention was for each actor to call each retailer no more than once. Given some actor attrition near the end of the calling period, we reassigned a few actors to call centres that they had previously called. There was no indication that any actor landed on the same call centre employee in any of the repeated calls.

We obtained residential addresses for our fictitious customers from an online website of homes available for rent. We separately randomly allocated addresses to customers within each retailer, so no retailer would be called twice with the same address.[19]

The calls took place in private offices in the University of Melbourne's Faculty of Business and Economics over the course of the third week of March 2017 between 9am and 4pm. The actors were provided with disposable SIM cards that they inserted into their own cell phones. Using cell phones enabled us to disable caller IDs.

Armed with a bargaining script, the caller dialled each designated retailer on speaker phone. A silent research assistant sitting next to them took duplicate notes on information revealed through the course of the call to ensure data quality.[20] The study's authors also participated silently in many calls to further ensure quality control and uniformity across calls. After each call, the actor and research assistant compared notes to finalise data collected from the call.

[18]Of the 17 retailers, we only contacted retailers who were manning call centres and offered postpaid electricity contracts. The retailers who were not contacted were very small retailers.

[19]Our full sample of calls has 395 calls because we initially planned to also vary home postcode (high vs. low-income) across all customer-retailer combinations. When we realized that phone calls were often stretching to the full time allotted, we decided to switch from duplicating addresses to randomizing them. Because we randomized the order of calls, duplicate treatments took place (with distinct addresses and callers) and are included in the final sample. Standard errors are clustered at the call level.

[20]Calls were not recorded as required by human ethics. Instead, all information were recorded on standardised price sheets. An example is attached in Appendix A.2.

As with previous audit studies, our experiment involved deception: retailers' call centre employees were not told that were participating in a study of retail price search and negotiation. Our actors were also briefed on the broader study context. To minimize the burden on call centre staff, we limited all calls to 20 minutes, and we encouraged actors to publicise good deals to friends and family after the experiment was run.

Standardising Customer Characteristics

There are many sources of electricity customer heterogeneity that we normalise to allow us to focus on the influence of search on retail price negotiations. In particular, we had actors represent customers with a two-bedroom rental apartment with an average monthly energy usage of 300 kWh/month. This corresponds to the average usage for a two-person consumer in Melbourne.

Home addresses for our fictitious customers were selected from 2-bedroom units available on a large online rental listing website.[21] All home addresses were chosen from the catchment of a single electricity distribution network, United Energy (www.unitedenergy.com.au/). This guaranteed that electricity network charges would be identical across all customers. We chose the United distribution network as their catchment area has the widest range of postcode-level consumer income according to the Australian Bureau of Statistics.

Our fictitious consumers were exclusively interested in electricity accounts without gas. Moreover, our consumers were not interested in green power plans nor time-of-day plans, which are relatively rare in the Victorian market. We also picked a uniform single rate electricity meter type, the most common meter type, for all customers.

Our consumers negotiated rates for a one-year contract. If asked, our callers explained that they had a one-year renewable lease. Finally, when we provide a reference price, we

[21] During our pilot calls, we learned that customers addresses were required to establish credibility with call centre personnel to initiate negotiations. The addresses also allowed us to also collect data on potentially perceived weekly rent, keeping in mind that the rent posted on the rental website does not specify contracted rent, which could be higher or lower than posted rent.

correctly associate the price with the same retailer(s), regardless of whether it is "high" or "low" reference price. We now provide specifics on our bargaining protocols.

Experimental Conditions

Each call consisted of two stages. In the first stage (*Initial Offer*) actors revealed their randomly-allocated address, subsidy status (e.g., experimental conditions *concession* or *not concession*), and whether they are moving into a new address in Melbourne or are a consumer of a rival retailer in Melbourne looking to switch (e.g., conditions *new consumer* or *switcher*). We picked the same rival for all of the switcher calls, who was not part of the experiment. The rival we picked displays its rates in proprietary units that make price comparisons very difficult. The choice of rival allowed our callers to easily deflect any questions about current rates.

Having provided this information, callers then wrote down the initial daily fixed charge and per kWh variable charge (or more simply, "price") offered by the retailer. We encouraged all actors to negotiate using total annual bills to facilitate comparison. The actors clarified any details regarding the offer, including what rates would be with or without discounts for direct-debit, pay-on-time, and paperless bills. We collected data on prices for each of these options, where available.

In the second stage of each call (*Negotiation*), actors reveal a reference price and how they obtained it. The reference price comes from one of four randomly-allocated sources: (1) previous call to one other company; (2) previous call to three other companies; (3) an online state government-run price-comparator website (https://compare.energy.vic.gov.au/); or (4) a friend. Below, we denote these information source-based conditions *called 1 rival*, *called 3 rivals*, *price comparator*, and *friend*. When the price came from previously-called companies, the name(s) of the companies were held fixed. Callers negotiated based on two reference price levels: a high and low price (e.g., conditions *high price* or *low price*). The "high" price was the lowest price obtained from the government-run online price-comparator website.[22] The "low" price was the lowest rate that we were able to negotiate over the phone during the pilot.

2.3.2 Data

Our dataset of retail prices contains default contract rates and posted price data from the field, and negotiated price data from our experiment. We obtained default rates and posted prices by scraping retail contracts from individual retailer websites. Each price quote is composed of a daily fixed charge and a per kWh variable charge, and any connection fees or special discounts. To normalise rates across retailers, we calculate a total annual bill based on average use of 300 kWh/month. Connection fees and discounts are included in the total annual bill estimate. All prices presented are before a 10% value-added-tax (VAT). Our callers were trained to confirm whether each quoted negotiated rate included or did not include VAT.

Summary Statistics

Summary statistics from our raw price data are presented in Table 2.1. They reveal substantial price dispersion: variable rates range from 14 to 40 cents per kWh, while daily electricity charges, which is the sum of daily fixed charges plus variable charges assuming 300 kWh/day consumption, range from 55 cents to $1.40 per day. Discounting is also notable from the summary statistics. Default contracts on average are 28.31 cents per kWh, falling to 27.35 cents per kWh for prices posted on firms' websites, then to 26.84 cents per kWh for initial prices offered during our experimental negotiations over phone, and then finally to 23.39 cents per kWh after stage two of our negotiations. That is, negotiating rates reduces prices 21% discount on average relative to default contracts.

[22] We checked each day of the experiment and confirm this price did not change.

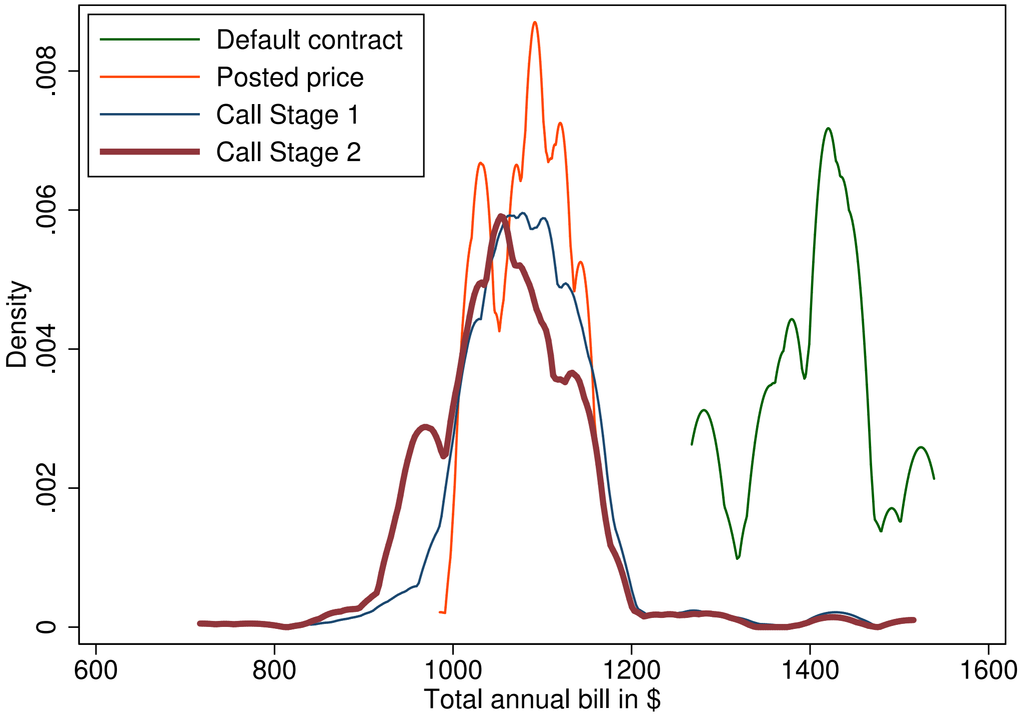
Table 2.1 Summary Statistics

	Mean	Std Dev	Min	Max	N
Cents per kWh					
Default contract	28.31	1.57	25.34	30.95	12
Posted price	27.35	2.63	20.69	29.63	12
Posted price, direct debit only	20.51	1.74	18.19	24.55	12
Call stage 1: *Initial*	26.84	3.22	17.15	39.57	395
Call stage 2: *Post-negotiation*	23.39	4.38	13.98	39.57	395
Reference price high	19.24				
Reference price low	17.85				
Cents per day					
Default contract	107.90	15.35	84.7	130.46	12
Posted price	107.47	14.29	81.31	130.46	12
Posted price, direct debit only	101.49	17.49	64.23	130.46	12
Call stage 1: *Initial*	107.64	14.69	73.92	148.45	395
Call stage 2: *Post-negotiation*	103.41	17.53	54.70	140.00	395
Reference price high	78.40				
Reference price low	70.50				

Figure 2.1 graphically presents the distribution of annual electricity costs implied from our prices dataset assuming 300 kWh energy consumption month. Here, we can see that annual bills from posted prices are centred near $1400 per year, whereas online posted market contracts and call stage 1 *Initial* offers are centred around $1150 per year. The distribution of call stage 2 *Post-negotiation* offers is shifted further to the left and is centred around $1050 per year, with a notable mass of prices to the left of $1000 per year which is not presented in the online market contracts nor the call stage 1 offers.[23]

The discounting in Figure 2.1 visually confirms significant price dispersion across the different groups of pricing contracts, as well as within these groups. On average, in our data there is a 22% reduction in prices when moving from default contracts to posted prices or stage 1 call-in offers. Moreover, posted prices appear to be the starting point within which firms start negotiating with consumers over the phone. Once consumers move past this

[23]In our sample, default prices are always worse than posted market prices and negotiated prices.



Notes: Default contract is the firm-specific default price ceiling. Posted price is the best rate posted on each retailers website. Call Stage 1 is the first price received by a customer moving into the market calling to connect service. Call Stage 2 is price received after customer reveals reference price and means and intensity of search.

Fig. 2.1 Observed Distribution in Prices for New Arrivals

initial price in stage 1 of the call, they are able to obtain an additional 10% discount off of their annual electricity bills. Below, we use regressions to formally estimate the degree of discounting between posted and negotiated prices.

Table 2.2 presents average annual total bills for each call-in treatment arm, where we continue to assume 300 kWh energy consumed per month. The table shows the per call lowest total annual bill averaged over retailers called. Although sometimes switchers receive lower rates, the effect is not strong when contrasted to new consumers in the market that have revealed search. The raw data also do not reveal strong evidence of concession holders being on higher rates conditional on search. Below, we use regressions to test for differences in rates across information sources, reference prices, concession status, and whether a consumer

Table 2.2 Average Total Annual bill Obtained under Each of the Treatment arms

		Not concession		Concession	
Information Source	Ref. Price	New	Switcher	New	Switcher
Called 1 rival firm	High	1076.97	1075.71	1067.47	1049.07
		(67.76)	(53.96)	(68.89)	(160.35)
	Low	1081.92	1044.25	1066.49	1038.67
		(72.81)	(57.60)	(69.54)	(69.84)
Called 4 rival firms	High	1071.65	1062.20	1097.90	1075.26
		(151.37)	(86.97)	(90.78)	(75.02)
	Low	1024.54	1048.79	1051.65	1052.29
		(98.58)	(71.36)	(61.09)	(51.93)
Price comparator	High	1080.11	1038.19	1115.79	1049.88
		(148.84)	(62.04)	(104.30)	(59.65)
Friend	High	1091.99	1049.11	1078.46	1068.75
		(123.68)	(49.19)	(85.11)	(51.91)
	Low	1050.73	1058.83	1068.78	1016.49
		(81.94)	(63.37)	(93.53)	(85.83)

Notes: Table shows per call lowest total annual bill based on 300 kWh/month usage, before VAT, averaged over all retailers called. Standard deviations in parentheses. "New" refers to a customer establishing a new connection. Price comparator refers to government-sponsored online price comparator website, https://compare.energy.vic.gov.au/ The high reference price is the lowest price observed on the government-sponsored price comparator website. The low reference price is the lowest negotiated rate obtained during pilot calls. The same retailer was (correctly) identified as the source of both sets of rates.

is new to the market. Importantly, these regressions account for caller and firm heterogeneity which partly drives the variation in rates in Table 2.2.

2.4 Experimental Results

In this section we present our experimental analysis of price posting and negotiation in the market. In all sections we compare posted prices and our experimental negotiated prices to the default contracts posted by firms. Section-by-section, we further examine variation in our negotiated offers, in particular how different experimentally-varied caller characteristics affect negotiation outcomes: (1) concessions vs. not concession; (2) new consumer vs.

switcher; (3) called 1 rival vs. called 3 rivals vs. price comparator vs. friend; and (4) high reference price vs. low reference price.

2.4.1 Baseline Results, New Consumers, and Switchers

While the across-call variation documented above is interesting in itself, it could reflect other systematic differences between new arrivals and switchers. For example, new arrivals could be perceived as de-sensitised to price given all of the very large expenses incurred during a move. The within-call variation controls for these other omitted factors, but cannot separately isolate willingness to search from the benchmarking properties of the reference price that is simultaneously provided.

To investigate the influence of being a switcher, and in all of our regressions that follow, we regress the log of the total annual bill for potential consumer i from retailer j, Bill_{ij}, calculated assuming total annual use of 3600 kWh, on the way the associated prices were obtained:

$$\begin{aligned}\log(\text{Bill}_{ij}) = &\beta_0 \text{PostedPrice}_j + \beta_1 \text{Stage1}_j + \beta_2 \text{Stage2}_j \\ &+ \beta_3 \text{Stage1}_j \times \text{Switcher}_i + \beta_4 \text{Stage2}_j \times \text{Switcher}_i \\ &+ \alpha_k + \rho_j + \delta_t + \kappa_n + \varepsilon_{ij} \qquad (2.1)\end{aligned}$$

where PostedPrice_j is the best price posted on retailer j's website. The omitted category is the default contract, also obtained from each retailer's website. Dummies for call stage 1: initial (Stage1_j) and call stage 2: post-negotiation (Stage2_j) represent the two rates collected during the call.[24] The dummy variable Switcher_i equals 1 if caller i claims to be interested in switching retailers and is not a new arrival to the market. Our regression also includes

[24] Recall that in stage 1, the quoted rate reflects whether the caller is a new arrival or a switcher, their residential address, and concession status. Further recall that the rate quoted in the second stage also reflects reference price and intensity of search revealed by the actor.

actor, retailer, and date-of-call fixed effects: α_k, ρ_j, and δ_t. We also include fixed effects for the cumulative number of calls made by each actor by the time of that call, n, to account for any actor-experience effects on negotiated offers. Standard errors are clustered at the retailer-method of search level.

In our base specification we highlight the difference between online and offline search. The coefficient β_1 captures the effect of calling in relative to signing up for service online. The coefficient β_2 captures the effect of revealing a reference price and intensity of search. β_3 measures whether a person calling to switch retailers, receives a lower initial rate relative to a market entrant establishing a new connection. Finally, β_4 measures whether the switcher receives an additional discount relative to a new entrant who has revealed search.

Table 2.3 presents baseline set of results, and the results for negotiation outcomes among switchers in the market. The sample we run this regression with includes two sets of price observations for each phone call (stage 1 and 2) and default contracts and posted prices for each retailer. In column (1), all bill reductions are shown relative to the default contract, which is the excluded category. The regressions in column (2) and all subsequent regressions in the essay drop the default contracts from the sample and specify posted prices as the excluded category. Bill reductions from our experimental negotiations within phone calls are then shown relative to posted prices.

We see that posted prices are on average 26% lower than default contracts. If consumers do not negotiate, those who are new consumers in the market who negotiate pay 1% higher rates than new consumers who signed up online. This implies that firms engage in negotiations with new consumers in the market at higher prices levels than what they offer them online.

Even ignoring connection fees, consumers that call a retailer to switch their service from a competitor are provided with an initial quote that is 1.9% lower than new consumers in the market. Although new consumers do best to sign-up online, existing switching consumers

Table 2.3 Baseline Results

	(1) log(Bill)	(2) log(Bill)
Market contract	-0.2565*** (0.004)	
Stage 1	-0.2458*** (0.006)	0.0101** (0.005)
Stage 1 X Switcher	-0.0200*** (0.006)	-0.0189*** (0.006)
Stage 2	-0.2680*** (0.007)	-0.0118** (0.005)
Stage 2 X Switcher	-0.0254*** (0.008)	-0.0246*** (0.007)
N	1,562	1,167
R^2	0.844	0.519

Notes: Dependent variable is log total annual bill based on 300 kWh/month usage, before VAT. In column (1) the omitted category is the standing contract. Column (2) drops the standing contract and presents reductions relative to posted prices. Standing contracts and posted prices were obtained from each retailer's website on the first day of the audit study, and did not change over the 10 day period over which calls were placed. Each phone call leads to two offers: stage 1 initial and stage 2 post-negotiation (after revealing reference price and mode and intensity of search). All regressions include actor, date, retailer, and number-of-calls-made-by-actors fixed effects. Standard errors are clustered at the call level.

do best to call and negotiate, even if they do no further negotiation. Indeed, switchers who negotiate receive an initial price that is, on net, lower than the best posted prices.

For new consumers in the market, revealing search and a reference price in stage 2 negotiation leads to rates that are 1.2% lower than market contracts available online. That is, new consumers can do better by negotiation. However, switchers can also obtain even larger price discounts from stage 2 negotiation, obtaining a total discount of 3.6% relative to the best prices available online.

Economic Magnitudes

All of these reductions are statistically-different from zero, but are they large? Recall from our discussion of institutional detail above, ACCC (2018c) estimates that the average annual bill in Victoria is \$1457 with a retail margin of 11%. From the Lerner Index, this implies an annual cost to serve of \$1297 per consumer. Conservatively applying our estimates of negotiation effects from switchers off of online market contracts, a 3.6% bill reduction yields average annual bill of \$1404. This implies a reduction in profit margin from negotiation among switchers in the market to 8%. In other words, retail profit margins decrease from 11 to 8 percentage points (pps), or by 30%, when consumers call in and negotiate rates. In this sense, the estimated impact of price negotiation on margins is economically large.

2.4.2 Reference Prices

To investigate the extent to which high vs. low references price influence negotiation outcomes, we re-run our regression from equation 2.1, interacting the Stage 2 price with whether or not the supplied reference price was high or low. The full regression is now:

$$\begin{aligned}\log(\text{Bill}_{ij}) =& \beta_1 \text{Stage1}_j + \beta_2 \text{Stage2}_j \\ &+ \beta_3 \text{Stage1}_j \times \text{Switcher}_i + \beta_4 \text{Stage2}_j \times \text{Switcher}_i + \beta_5 \text{Stage2}_j \times \text{Low price}_i \\ &+ \beta_6 \text{Stage2}_j \times \text{Switcher}_i \times \text{Low price}_i + \alpha_k + \rho_j + \delta_t + \kappa_n + \varepsilon_{ij} \qquad (2.2)\end{aligned}$$

where Low price$_i$ equals one if consumer i negotiates with the low reference price in stage 2 of the negotiation process, and is 0 otherwise (e.g., they negotiate with the high price).

Table 2.4 presents our empirical results. The estimate of β_5 in column (2), that is the effect of providing a low reference price when revealing search, leads to significantly lower price quotes in the second stage. Low reference prices lead to negotiated rates that are 1.8% lower than posted prices. We further find that these second-stage negotiated reductions in

Table 2.4 Reference Prices

	(1) log(Bill)	(2) log(Bill)
Stage 1	0.0101**	0.0102**
	(0.005)	(0.005)
Stage 1 X Switcher	-0.0189***	-0.0189***
	(0.006)	(0.006)
Stage 2	-0.0118**	-0.0034
	(0.005)	(0.007)
Stage 2 X Switcher	-0.0246***	-0.0310***
	(0.007)	(0.010)
Stage 2 X Low Price		-0.0180**
		(0.009)
Stage 2 X Switcher X Low Price		0.0133
		(0.011)
N	1,167	1,167
R^2	0.519	0.523

Notes: Dependent variable is log total annual bill based on 300 kWh/month usage, before VAT. The omitted category is the posted price. Posted prices are obtained from the retailer's website. Each phone call leads to two offers: stage 1 initial and stage 2 post-negotiation (after revealing reference price and mode and intensity of search). The high price is the lowest posted price obtained from the government-sponsored price comparator website. The low price is the lowest negotiated rate obtained during pilot calls. The same retailer was (correctly) identified as the source of both sets of rates. All regressions include actor, date, retailer, and number-of-calls-made-by-actors fixed effects. Standard errors are clustered at the call level.

price are driven by calls made either by switchers or by new consumers in the market who quote low reference prices. The results in column (2) continue to show that new consumers who fail to quote low reference prices otherwise realise negotiated prices that are higher than what could be obtained from prices posted online.

We note that the high reference price is only high relative to the lowest price a firm is willing to offer through price negotiation. Our experimental high price is indeed considerably lower than many prices posted on individual retailer websites; recall that the high price is the lowest posted price identified by the government-sponsored price comparator website, which coarsely reflects the rates available on individual retailer website, whereas the low price is the lowest negotiated rate obtained during pilot calls. In no case did a caller receive an initial price quote in Stage 1 that was lower than the high reference price cited in our negotiations.

2.4.3 Source of Price Information

Does the source of price information matter for negotiations? We now investigate whether our conditions for having called 1 firm, 3 firms or having asked a friend about their prices matters, conditional on citing either a low or high price in negotiations. To investigate such heterogeneity in stage 2 negotiation outcomes, we run analogous regressions to our baseline regression in (2.1) above, but include appropriate interactions between our dummies for stage 2, switchers, and low price with dummy variables for whether a consumer called one company, three companies, or asked their friends.

Sequential Search

Varying whether a customer has previously called one versus four competitors effectively varies a consumer's position in a sequential search process as in McCall (1970). Holding consumer characteristics and their reference price fixed, a consumer who has called just one competitor has a higher continuation value from searching for additional price quotes than a consumer who has called three competitors and who thus has fewer future price quotes from other firms to potentially draw. If firms negotiate prices based on individuals' continuation values, and if they attempt to pre-empt future competitors' prices by offering lower prices to captive consumers whom they are currently negotiating with (e.g., as in the structural model of Allen et al. (2018)), then we expect that firms would offer a lower price quote to consumers who have previously searched at just one competitor to overcome their relatively higher continuation value of additional search. By comparing price quotes from consumers who have searched at one versus three firms previously, we can assess whether such search-based pre-emptive pricing exists.

To be explicit, we implement the test by modifying our regression equation as follows:

$$\begin{aligned}\log(\text{Bill}_{ij}) =& \beta_1 \text{Stage1}_j + \beta_2 \text{Stage2}_j \\ & + \beta_3 \text{Stage1}_j \times \text{Switcher}_i + \beta_4 \text{Stage2}_j \times \text{Switcher}_i + \beta_5 \text{Stage2}_j \times \text{Low price}_i \\ & + \beta_6 \text{Stage2}_j \times \text{Switcher}_i \times \text{Low price}_i + \beta_7 \text{Stage2}_j \times \text{Switcher}_i \times \text{Low price}_i \times \text{Called } 1_i \\ & + \beta_8 \text{Stage2}_j \times \text{Switcher}_i \times \text{Low price}_i \times \text{Called } 3_i + \alpha_k + \rho_j + \delta_t + \kappa_n + \varepsilon_{ij}\end{aligned} \tag{2.3}$$

where Called 1_i and Called 3_i are dummies that respectively equal one if consumer i has previously called 1 or 3 firms in obtaining their price quote.

Table 2.5 present our results from these tests. There is some evidence of pre-emptive search-based pricing: consumers who have just called one firm realise price discounts, whereas consumers who have called three firms fail to realise discounts and instead pay higher prices, holding other factors fixed. We also find large magnitude differences in the discounts, though the coefficients are not statistically different from each other, which could be due to low power.

Cheap Talk

The final variation on information source for reference prices we consider claiming the quote was provided by “a friend”. If firms perceive this non-market based information as cheap talk, they may downweigh such price claims and not engage in negotiations based on such prices.

Table 2.5 Sequential Search Pre-emptive Pricing

	(1) log(Bill)	(2) log(Bill)	(3) log(Bill)
Stage 1	0.0102**	0.0101**	0.0100**
	(0.005)	(0.005)	(0.005)
Stage 1 X Switcher	-0.0189***	-0.0187***	-0.0186***
	(0.006)	(0.006)	(0.006)
Stage 2	-0.0034	-0.0035	-0.0035
	(0.007)	(0.007)	(0.007)
Stage 2 X Switcher	-0.0310***	-0.0276***	-0.0347***
	(0.010)	(0.009)	(0.010)
Stage 2 X Low Price	-0.0180**	-0.0180**	-0.0180**
	(0.009)	(0.009)	(0.009)
Stage 2 X Switcher X Low Price	0.0133	0.0111	0.0145
	(0.011)	(0.012)	(0.013)
Stage 2 X Switcher X Low Price X Called 1		-0.0036	
		(0.011)	
Stage 2 X Switcher X High Price X Called 1		-0.0120	
		(0.016)	
Stage 2 X Switcher X Low Price X Called 3			0.0080
			(0.010)
Stage 2 X Switcher X High Price X Called 3			0.0154
			(0.013)
N	1,167	1,167	1,167
R^2	0.523	0.523	0.524

Notes: Dependent variable is log total annual bill based on 300 kWh/month usage, before VAT. The omitted category is the posted price. Posted prices are obtained from the retailer's website. Each phone call leads to two offers: stage 1 initial and stage 2 post-negotiation (after revealing reference price and mode and intensity of search). The high price is the lowest posted price obtained from the government-sponsored price comparator website. The low price is the lowest negotiated rate obtained during pilot calls. The same retailer was (correctly) identified as the source of both sets of rates. All regressions include actor, date, retailer, and number-of-calls-made-by-actors fixed effects. Standard errors are clustered at the call level.

To test this using our experimental data, we modify our regression equation as follows:

$$
\begin{aligned}
\log(\text{Bill}_{ij}) =& \beta_1 \text{Stage1}_j + \beta_2 \text{Stage2}_j \\
& + \beta_3 \text{Stage1}_j \times \text{Switcher}_i + \beta_4 \text{Stage2}_j \times \text{Switcher}_i + \beta_5 \text{Stage2}_j \times \text{Low price}_i \\
& + \beta_6 \text{Stage2}_j \times \text{Switcher}_i \times \text{Low price}_i + \beta_7 \text{Stage2}_j \times \text{Switcher}_i \times \text{Low price}_i \times \text{Friend}_i \\
& + \beta_8 \text{Stage2}_j \times \text{Switcher}_i \times \text{High price}_i \times \text{Friend}_i + \alpha_k + \rho_j + \delta_t + \kappa_n + \varepsilon_{ij} \quad (2.4)
\end{aligned}
$$

where Friend_i is a dummy variable equalling one if consumer i claims to have obtained the price information from a friend.

We present our findings in Table 2.6. The estimates of β_7 and β_8 from our regression in (2.4) are very small in magnitude and statistically insignificant. That is, we find minimal differences in firms' price discounting in negotiations if consumers simply claim their price information comes from a friend and not the market such as from having search rival firms. What matters in negotiations in the market is negotiating under the threat to switch retailers and having a low reference price in doing so; the credibility of the source of the information is irrelevant.

2.4.4 Passthrough of Government Subsidies

As discussed in Section 4.2, the national and state governments are importantly concerned with the fact that low-income consumers tend to not engage in search and price negotiation to obtain competitive electricity contracts, and therefore on average pay higher base rates than other consumers for a non-differentiated and essential product in electricity. (ACCC, 2018c; Thwaites et al., 2017). To help alleviate this issue, the state government of Victoria subsidises 17.5% of the electricity bill of low-income concession consumers.

In this final section we leverage our experiment to investigate whether concession holders tend to receive higher initial price quotes when searching and negotiating for electricity

Table 2.6 Cheap Talk

	(1) log(Bill)	(2) log(Bill)	(3) log(Bill)
Stage 1	0.0102**	0.0102**	0.0102**
	(0.005)	(0.005)	(0.005)
Stage 1 X Switcher	-0.0189***	-0.0190***	-0.0190***
	(0.006)	(0.006)	(0.006)
Stage 2	-0.0034	-0.0034	-0.0033
	(0.007)	(0.007)	(0.007)
Stage 2 X Switcher	-0.0310***	-0.0311***	-0.0323***
	(0.010)	(0.010)	(0.011)
Stage 2 X Low Price	-0.0180**	-0.0180**	-0.0180**
	(0.009)	(0.009)	(0.009)
Stage 2 X Switcher X Low Price	0.0133	0.0151	0.0163
	(0.011)	(0.012)	(0.012)
Stage 2 X Switcher X Low Price X Friend		-0.0051	-0.0052
		(0.011)	(0.011)
Stage 2 X Switcher X High Price X Friend			0.0051
			(0.011)
N	1,167	1,167	1,167
R^2	0.523	0.523	0.523

Notes: Dependent variable is log total annual bill based on 300 kWh/month usage, before VAT. The omitted category is the posted prices. Posted prices are obtained from the retailer's website. Each phone call leads to two offers: stage 1 initial and stage 2 post-negotiation (after revealing reference price and mode and intensity of search). The high price is the lowest posted price obtained from the government-sponsored price comparator website. The low price is the lowest negotiated rate obtained during pilot calls. The same retailer was (correctly) identified as the source of both sets of rates. All regressions include actor, date, retailer, and number-of-calls-made-by-actors fixed effects. Standard errors clustered at the call level.

contracts. That is, we are able to test for the rate of passthrough of government concessions to retail electricity prices paid by low–income consumers. If firms are systematically negotiating from higher price levels for concession consumers, this is consistent with them exercising market power in extracting rents related to the 17.5% concession subsidy provided by government. For our investigation, we run the following regression:

$$\begin{aligned}
\log(\text{Bill}_{ij}) = &\beta_1 \text{Stage1}_j + \beta_2 \text{Stage1}_j \times \text{Switcher}_i + \beta_3 \text{Stage1}_j \times \text{Concession}_i \\
&+ \beta_4 \text{Stage1}_j \times \text{Switcher}_i \times \text{Concession}_i \\
&+ \beta_5 \text{Stage2}_j + \beta_6 \text{Stage2}_j \times \text{Switcher}_i + \beta_7 \text{Stage2}_j \times \text{Concession}_i \\
&+ \beta_8 \text{Stage2}_j \times \text{Switcher}_i \times \text{Concession}_i + \alpha_k + \rho_j + \delta_t + \kappa_n + \varepsilon_{ij}
\end{aligned}$$

where Concession_i is a dummy equalling one if consumer i claims they have concession status. The difference in specification in interacting the Concession_i dummy with stage 1 and not stage 2 variables arises because recall in our design that we reveal concession status in stage 1 (Initial Price) of negotiations.

Table 2.7 presents our results. None of the coefficients on the concession-related variables are large nor are they statistically significant. This is true even for new consumers in the market who have not yet revealed their search behaviour and corresponding reference prices. In sum, our experiment reveals that firms are not exercising market power to recoup some of the governments' concession payments either in initial prices offered during stage 1 of negotiations, or in final prices realized after consumers reveal their search behaviour and reference prices.

Table 2.7 Concession Status

	(1) log(Bill)	(2) log(Bill)
Stage 1	0.0101**	0.0095
	(0.005)	(0.007)
Stage 1 X Switcher	-0.0189***	-0.0178*
	(0.006)	(0.010)
Stage 1 X Concession		0.0011
		(0.008)
Stage 1 X Switcher X Concession		-0.0019
		(0.011)
Stage 2	-0.0118**	-0.0159**
	(0.005)	(0.008)
Stage 2 X Switcher	-0.0246***	-0.0177*
	(0.007)	(0.010)
Stage 2 X Concession		0.0084
		(0.009)
Stage 2 X Switcher X Concession		-0.0138
		(0.013)
N	1,167	1,167
R^2	0.519	0.520

Notes: Dependent variable is log total annual bill based on 300 kWh/month usage, before VAT. The omitted category is the posted price. Posted prices are prices on the retailer's website. Each phone call leads to two offers: stage 1 initial and stage 2 post-negotiation (after revealing reference price and mode and intensity of search). Concession status is revealed in Stage 1. No caller was asked what qualified them for concession status (health care card = low-income, elderly, veteran). All regressions include actor, date, retailer, and number-of-calls-made-by-actors fixed effects. Standard errors are clustered at the call level.

2.5 Conclusion

An emerging body of empirical research in IO is revealing the value of administrative data on consumer-firm specific prices for examining market power and retail price dispersion and their underlying causes: price discrimination, search frictions, and negotiation. While this research is providing new frameworks to inform our understanding of the issues in oligopolistic pricing, their general applicability is potentially limited due to the sensitive nature of the administrative data that these frameworks require.

Through this study, we have developed an audit study to examine price discrimination, search, and negotiation in a retail market. Our simple, yet novel and powerful methodology for examining these issues complement emerging structural approaches based on administrative data. The simplicity of our approach can be immediately implemented by government agencies looking to identify market power and its sources in industries where there is substantial consumer-firm specific heterogeneity in prices, with the aim of developing policy interventions to circumvent market power and promote efficiency such as government-provided online price search platforms. Furthermore, in this context where firms have some degree of market power, price negotiation by consumers with different search costs has different implications on efficiency. For instance, it is likely that consumers with higher search costs have lower elasticity of demand for electricity usage. Therefore, charging a higher price to this group of consumers will result in greater efficiency loss than charging a higher price to a group of consumers with higher elasticity of demand.

Our empirical results shed new light on how firms engage in price discrimination based on search frictions. Firms offer higher prices and are less willing to engage in price negotiation with new consumers in markets compared to existing consumers threatening to switch firms. To our knowledge, this distinction between de novo consumers and existing consumers in a market as a source of discriminatory pricing has previously gone undocumented.

We further find that the level of reference prices in negotiations are, perhaps unsurprisingly, important for discounts consumers can achieve through retail price negotiation. Lower reference prices result in lower negotiated prices. Yet, the source of the information for reference prices, whether it comes from sequential search in the market or cheap talk, is irrelevant for negotiated price outcomes.

Finally, our experiment is able to reveal competitive features of a market that were previously not well-understood. We find no evidence of incomplete pass-through of government subsidies for low-income consumers in price negotiations. We also document a statistically significant and economically large impact of retail price negotiation on market power. In our experimental context, an existing consumer who is negotiating with a low reference price is able to reduce average profit margins by approximately 30%, from a mark-up of 11 to 8 percentage points. That is, the market we study appears to be relatively competitive among the subset of fully engaged shoppers. This further underscores on-going policy measures aimed at overcoming the search frictions consumers face in negotiating prices, which have persisted as an underlying source of market power in the market we study for more than 10 years since the market deregulated.

Chapter 3

Information Disclosure and Price Coordination in Retail Gasoline Markets

3.1 Introduction

This essay studies the role of mandatory information disclosure platforms on market competition based on the following policy. On August 1, 2016, the state government of New South Wales, Australia, introduced a state-wide legislation that requires every single gas station to post prices on an online platform known as FuelCheck. These prices are then made available to the general public, who can locate the lowest priced station by entering their suburb or postcode on the website. This is a classic example of a demand-side technology that lowers search frictions, which should increase demand elasticities and lower prices.

However, information disclosure policies also make it easier for firms to monitor their competitors, which raise concerns about tacit collusion. The equilibrium impact would then depend on which effect dominates. For instance, Rossi and Chintagunta (2016) find that when fuel prices are posted on large electronic signs along Italian highways, average prices decrease. However, Albæk et al. (1997) find that prices of ready-mixed concrete in Denmark increased following the Danish antitrust authority's decision to publish firm-specific prices.

Similarly, Jang (2014) and Luco (2018) also find evidence of anti-competitive conduct under mandatory price reporting policies in retail gasoline markets. While all these papers find evidence of anti-competitive conduct, none of them has demonstrated how margin-enhancing effects are achieved or how firms interact with one another under the influence of mandatory information disclosure.

This essay is structured in two parts. In the first part, I conduct a low frequency margin analysis of the mandatory information disclosure policy in New South Wales' (NSW) retail gasoline market using monthly price data across a large cross-section of markets. For this estimation, I use a panel of monthly average retail and wholesale prices for 93 markets in Australia that include markets that are affected and not affected by the policy and span both pre- and post-intervention periods. In addition, I exploit the rich cross-section of markets to explore the heterogeneous effects of the policy using a difference-in-differences strategy. Overall, I find that the policy led to margin-enhancing effects; average price-cost margins increased by 14.4% to 18.8% after the policy was introduced. I also find that these effects are heterogeneous across markets. In fact, margin-enhancing effects are only observed in small regional cities but not in larger cities. Since the change in margins could be driven by demand- and supply-side forces, I also obtain auxiliary search data from the online platform in order to control for margin effects that are driven by search on the online platform. However, since the online platform's adoption rate is only 1% at the time of writing this essay, I focus the rest of the analysis on supply-side forces.

While the findings that show that the policy has led to margin-enhancing effects is interesting in itself, a natural question would be to ask: what behaviour might have led to the increase in margins? Therefore, in the second part of this essay, I conduct a high frequency price analysis to understand the mechanisms that drive margin increases observed in the low frequency margin analysis. To investigate this, I obtain real-time station-level prices from the post-intervention period for all gas stations located in the markets affected by the policy. Interestingly, my findings reveal a state-wide transition in pricing equilibrium from price cycles to fixed prices immediately after the policy was introduced in regional markets. Coles, who is one of the market leaders, successfully communicated this transition to their rivals through prices. By linking the results from the low frequency margin analysis and high frequency price analysis, I show that the transition in price equilibrium explains the margin increases, which raises concerns that information disclosure facilitates tacit collusion.

In this sense, the low frequency margin analysis in this essay most closely relates to the study in Luco (2018). Both our studies explore the impact of information disclosure on retail margins in retail gasoline markets and we both find evidence of information disclosure policies leading to margin-enhancing effects. The main differences between our studies are twofold. First, his analysis only focuses on urban markets whereas my study covers urban and regional markets. Because margins only increased in regional markets but not in urban markets, my results suggest that price disclosure policies may have important distributional effects. Second, the availability of high frequency price data enable me to further investigate the underlying mechanisms of the margin increases, which is not documented in Luco (2018)'s paper.

The high frequency price analysis in this essay highlights the importance of using high frequency data to reveal potential anti-competitive conduct. In the absence of real-time data, Coles' price leadership may be masked since retail gas prices change multiple times a day in this market. Another recent study that uses high frequency data to reveal gas station conduct is the study by Byrne and De Roos (2019). Using real-time station-level data in Perth, Australia, they reveal that BP used "Wednesday price jumps" as a message to communicate with their rivals about transitioning the market to a focal point equilibrium.[1] My study that shows Coles using prices to communicate a state-wide transition to a more profitable

[1] The use of focal points as a tool for tacit collusion has also been documented in the following papers. Foros and Steen (2008) find that gas stations in Norway use the recommended price as a coordination device, while Lewis (2015) finds that gas stations set odd ending prices as focal points to facilitate tacit collusion.

equilibrium adds new evidence to the literature that explores the use of prices as a vessel for tacit coordination.

Retail gasoline is suitable for studying tacit collusion. It meets all the criteria for cartel formation and survival as stipulated in Stigler (1964)'s paper, such as homogeneity of products, buyers, and sellers; a small number of sellers but a large number of buyers; minimal non-price competition; and transparency of prices. Due to its susceptibility to collusive practices, retail gasoline markets have inspired investigations by antitrust authorities in many countries.

My essay also contributes to the literature that evaluates the impact of information disclosure using pre- and post-intervention data. The papers in this area are limited, primarily because price data are usually not available in the pre-intervention period. There are a few exceptions, such as Luco (2018) and Ater and Rigbi (2018), who study the impacts of mandatory information disclosure policies in retail gasoline and retail grocery industries. The availability of pre- and post-intervention data in these papers allows the authors to estimate the causal impacts of the policies. Given that pre-intervention data are difficult to obtain, there are also papers that only use post-intervention data and these papers are therefore limited to making non-causal interpretations. Examples include Jang (2014), Albæk et al. (1997) and Byrne and De Roos (2019).

The remainder of this essay is organised as follows. Section 3.2 discusses the context of this essay. Section 4.3 describes the data used for this essay's analysis. Section 3.4 provides details on the estimation strategy. Section 3.5 discusses the results of this essay. Section 4.6 concludes.

3.2 Context

3.2.1 Industry

The retail gasoline industry in Australia is highly concentrated, consists of asymmetric firms, and is dominated by a few vertically-integrated oil companies and supermarkets[2]. The largest four retailers, BP, Caltex, Coles and Woolworths[3] have retail market shares of 19%, 17%, 19% and 18% in the country (ACCC, 2018d). The remaining 27% of the market share is divided amongst a large number of stations operated by mid-tier and independent retailers who compete aggressively on prices with little non-price differentiation. Usually, individual retailers set their own prices unless the retailer is centrally owned by an oil major or supermarket.

3.2.2 Informed Sources

In most countries, except Australia, it is illegal for retailers to share information exclusively amongst each other. For more than a decade, major gas retailers in Australia have been sharing price information at near real-time frequencies with each other through a service provided by Informed Sources.[4] This service is exclusive to subscribers, which means that only a handful of retailers can perfectly observe each others' prices.[5]

On the August 19, 2014, the Australian antitrust authority (ACCC) instituted Federal Court proceedings against Informed Sources and five gas retailers (BP, Caltex, Coles, Woolworths and 7-Eleven) amid concerns that the retailers were using Informed Sources' service

[2] In recent years, supermarkets have transformed the retail gasoline industry in several countries. For example, supermarkets account for 56% of the gasoline sales in France and 28% in the United Kingdom (Gauthier-Villars, D, 2004)

[3] Coles and Woolworths are supermarkets and offer "shopper docket" discounts of 4 Australian cents per litre that tie gas and grocery purchases. They purchase gas exclusively from Shell and Caltex respectively. BP and Caltex are refiner-marketers.

[4] Although the actual commencement date of the Informed Sources' price-sharing service is not available, industry reports suggest that this service has been operating even before 2007 (ACCC, 2007).

[5] The ACCC (2007) reports that Informed Sources collects prices of 3500 gas stations in Australia, which is approximately half the total number of stations in the country.

to coordinate on prices. Coles terminated their subscription with Informed Sources at the end of their contract in April 2016 and charges against them were dropped. Subsequently, the case involving the other retailers was resolved without fines or penalties, and Informed Sources was allowed to continue providing the same service to subscribers. The only condition imposed on subscribers was a five year commitment to provide Informed Sources' data to consumers via a free platform, and to make the data available to third party commercial entities on reasonable terms (ACCC, 2017).

3.2.3 FuelCheck

On August 1, 2016, the NSW state government launched a state-wide price transparency initiative known as FuelCheck. The policy was rolled out simultaneously across the state of NSW and affects all gas stations in the state. It provides motorists access to real-time price information of gas stations located in the suburb or postcode that they specify on FuelCheck's website.[6]

Aside from FuelCheck, there are a few privately-operated price transparency platforms that provide similar services for consumers, such as Motormouth, PetrolSpy and GasBuddy, except that they rely on crowdsourced data.[7] This implies that FuelCheck is the only platform that lines up physical and digital prices by law. Moreover, the prices found on privately-owned platforms are likely to be lagged by hours or even days, which means that FuelCheck is likely the dominant driver of transparency in this context. Furthermore, the state government of Queensland in Australia adopting its own legislated price transparency platform is further evidence of the importance of government-run sites.

[6] By law, gas station owners are required to update prices on the website as soon as they are updated at the pump. The NSW government conducts checks and imposes a sanction of $550 on gas station owners who misreport prices.

[7] These apps have been operating in the Australian market before FuelCheck was introduced. Motormouth was introduced in July 2000, PetrolSpy was launched in March 2014 and GasBuddy, an American-based app, was introduced in March 2016.

3.3 Data

My essay explores the effects of information disclosure on market competition evolves in two parts: 1) Low frequency margin analysis and 2) High frequency price analysis. For these analyses, I use datasets containing information on market-level retail prices, wholesale prices, station-level prices and auxiliary search data from FuelCheck's online platform.

3.3.1 Low Frequency Data: City-level Monthly Prices

The dataset used in my main analysis contains a panel of monthly average retail prices of regular unleaded petrol in 114 cities across Australia from April 1998 to June 2017. They are obtained from Fueltrac, a petroleum industry specialist who collects and tracks retail prices nationwide. This dataset is suitable for my study because it contains pre-intervention and post-intervention retail prices in a large cross-section of markets that are affected and not affected by the policy.

From the original data, I narrow down my sample to only include retail prices between October 2015 and June 2017. I do this because there are many missing values in the data in the earlier periods. Apart from that, I also exclude markets in the Northern Territory, Cairns and Armidale due to ongoing petrol price inquiries that coincide with the timing of the policy. I also remove markets in Western Australia because gas retailers are governed by a price commitment policy that is unique to this state. In total, the final sample contains 1953 market-level monthly average prices from 93 markets located in the Australian states of New South Wales (NSW), South Australia (SA), Victoria (VIC) and Queensland (QLD).[8] Only markets in NSW are affected by FuelCheck from August 2016.

I supplement the market-level retail prices with wholesale prices for the purpose of computing margins as a measure of competition. The Australian Institute of Petroleum publishes daily average Terminal Gate Prices (TGP) of regular unleaded petrol in the capital

[8]The data for Australian Capital Territory and Tasmania were not available at the time this essay was written.

cities of Australia. I obtain these daily average TGP's from October 2015 to June 2017 for Sydney, Adelaide, Melbourne and Brisbane and aggregate them to monthly averages. Due to large transportation costs, I assume that gas stations purchase refined gasoline products from terminal gates in the capital city of their own state. Next, I compute monthly average retail margins in each city by taking the difference between retail and wholesale prices.[9] The unit of observation in the low frequency margin analysis of this essay is the average monthly retail margin in a given market.

3.3.2 High Frequency Data: Station-level Real-time Prices

The second main dataset consists of the universe of retail price updates reported on FuelCheck's website between August 1, 2016 to June 30, 2017. Since price reporting is mandatory, this means that I have real-time prices of all gas stations in the state of NSW. I obtain from this dataset the timestamp and retail prices of all regular unleaded petrol price updates in the markets affected by the policy. In addition, I also identify the gas station responsible for uploading each price by obtaining their address and brand. Using addresses, I match gas stations to their respective markets and include observations that fall within the markets studied in the low frequency margin analysis. In total, my sample contains 131,239 price updates from 959 gas stations. Having access to these high frequency price updates is useful because it helps me identify market dynamics in the retail gasoline context, such as price leadership and price coordination, which are impossible to identify using aggregate data.

3.3.3 Search Data

The final dataset used in my analysis contains information on search requests executed on FuelCheck's online platform. Specifically, I have access to the timestamp and geocoded

[9]All retail and wholesale prices are expressed in real terms and have been deflated by inflation with 2012 as its base year. Also note that the definition of retail margins here do not include operating costs.

location of 1,281,969 search queries on FuelCheck from September 17, 2016 to March 8, 2017. For each query, I match the geocoded location to the markets in my sample. I then aggregate the total number of search queries made in each month and market[10] as my measure of monthly search intensity.[11] With the availability of search intensity data across markets, I can control for consumer search in assessing conduct.

3.3.4 Descriptive Statistics of Margins and Consumer Search

Table 3.1 tabulates the mean and standard deviation of monthly average real margins before the intervention (October 2015 – July 2016) and after the intervention (August 2016 – June 2017). All margins and prices mentioned in this essay are reported in Australian cents per litre (cpl). Average margins are higher in the pre-intervention period than in the post-intervention period in South Australia, Victoria and Queensland, except for New South Wales. In NSW, in contrast, average margins have increased from 13.26 cpl to 14.58 cpl in the post-intervention period in New South Wales. These descriptive statistics suggest that there is a possibility that the price transparency platform implemented in New South Wales has led to an increase in average real margins.

The markets in the sample are summarised in Table 3.2. Victoria has the largest average population size, followed by New South Wales, Queensland and South Australia. The summary statistics highlight large variations in population sizes across the markets. In all four states, the standard deviations of population size are between two to four times larger than the average population size in the state. The population distribution in each state is also positively skewed, which suggests that the sample mostly consists of small markets with the exception of a few very large markets. These variations in city sizes presents me the

[10] I exclude observations in the months of September 2016 and March 2017 because the data are incomplete

[11] In the absence of a person identifier in my data, I follow the precedent set out by other papers in the literature (Byrne and de Roos (2017) and Luco (2018)) and assume that each search query is made by a different individual.

Table 3.1 Descriptive Statistics of Average Real Margins

		Average Real Margins (cpl)			
		NSW	SA	VIC	QLD
Oct 2015 to Jul 2016	*Mean*	13.26	13.14	12.19	15.43
	Std. dev.	(5.16)	(6.94)	(4.55)	(5.78)
Aug 2016 to Jun 2017	*Mean*	14.58	12.00	11.30	15.18
	Std. dev.	(4.98)	(6.61)	(3.15)	(5.77)

Notes: Average margins are the inflation-adjusted margins (in Australian cents per litre), computed as the difference between average market-level retail price and average wholesale price in month-year *t* (e.g. January 2017). The sample includes 93 markets and the sample period extends from October 2015 to June 2017.

Table 3.2 Summary Statistics of Cities in the Aggregate Data

		Population Size				
	No. of Markets	Mean	Std. dev.	Median	p5	p95
New South Wales	30	181076	772649	17852	275	4321534
South Australia	11	117680	332194	14062	1625	1165639
Victoria	24	203512	834120	14569	1947	4196201
Queensland	28	119788	377603	18342	934	2054616

Notes: Summary statistics are computed using population data obtained from the ABS (2016)'s census via the TableBuilder function. The sample includes 93 markets.

opportunity to study the differential effects of the information disclosure policy in urban and regional markets.

In this environment with perfect observability of prices, consumer search and price coordination potentially coexist. Therefore, to check if search potentially plays a role in explaining the policy effects, I show the distribution of monthly per capita search intensity on FuelCheck's website between October 2016 to February 2017 in Figure 3.1, across markets grouped in population quintiles. Descriptive statistics of per capita search intensity on FuelCheck's website are also provided in Table 3.3. Taken together, it appears that FuelCheck's adoption rate in this period is low at only 1%, apart from markets ranked in

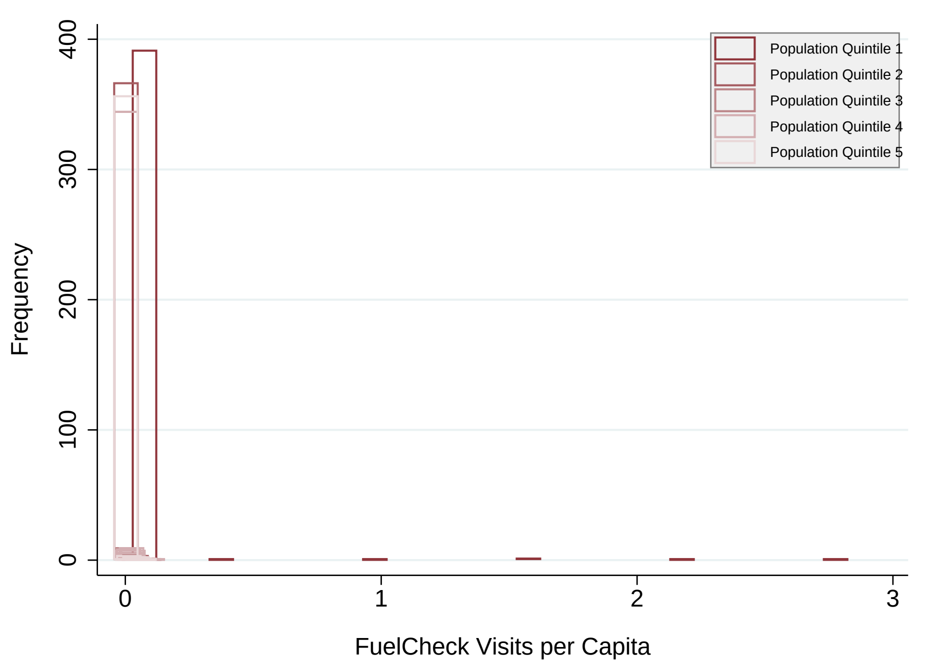


Fig. 3.1 Distribution of FuelCheck Website Visits per Capita

the lowest population quintile. Given this adoption rate, it is unlikely that consumer search activities on FuelCheck's website will drive this essay's main results, hence the remainder of this essay will focus on supply-side factors.

3.4 Empirical Strategy

3.4.1 Research Design

The availability of pre- and post-intervention data allows for estimating the causal effect of the policy on market outcomes. In my baseline analysis, I use a standard difference-in-differences (DiD) estimation strategy. I compare markets that are affected by the policy against markets that are unaffected by the policy, both before and after the intervention. My sample consists of 30 cities in NSW that form the "treatment group" and 63 markets in SA, VIC and QLD that form the "control group".

Table 3.3 Descriptive Statistics of FuelCheck Website Visits per Capita

Pop. Quintile	Month				
	Oct 2016	Nov 2016	Dec 2016	Jan 2017	Feb 2017
1	0.12	0.09	0.06	0.16	0.09
	(0.51)	(0.36)	(0.22)	(0.65)	(0.36)
2	0.01	0.01	0.01	0.01	0.00
	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)
3	0.00	0.00	0.01	0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
4	0.01	0.01	0.01	0.03	0.01
	(0.02)	(0.02)	(0.02)	(0.03)	(0.01)
5	0.01	0.01	0.01	0.01	0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)

Notes: This table reports the mean and standard deviation (in parentheses) of total website visits per capita in a given month. The sample consists of 93 markets. The sample period includes October 2016 – February 2017.

The key identifying assumption for this study is that the parallel trends assumption holds. I argue that markets in SA, VIC and QLD are suitable comparison groups for markets in NSW due to three reasons. First, retail gasoline prices are largely determined by changes in global crude oil prices and exchange rate fluctuations. Therefore, any macroeconomic shocks to Australia should affect all markets in the same way. Second, the markets in all states are highly concentrated and dominated by "C4" firms – BP, Caltex, Coles and Woolworths, which implies similar market structure nationwide. Third, to the best of my knowledge, there were no state-specific shocks or mergers during my sample period that would have differentially impacted some of the markets (ACCC, 2017). Moreover, having access to market-level data allows me to control for unobserved heterogeneities across markets by including market fixed effects. In addition, my sample that contains data from multiple pre-intervention periods enable me to examine if any pre-trend differences exist.

In spite of the above, there are still a few concerns that should be addressed. First, Coles' withdrawal from the Informed Sources subscription service in April 2016 before the price disclosure policy was introduced could bias the average treatment effects of the policy. Their withdrawal effectively reduced the number of firms that were perfectly informed of their rivals' prices. If, as a result of Coles' withdrawal, coordination became harder to sustain, average margins might be lower during this period. This could lead to an upward bias of the DiD estimates. So, as a robustness check, I re-run my main regressions and exclude observations in April 2016 to July 2016 (inclusive). Another caveat relates to the presence of other price transparency apps for retail gasoline, as discussed in Subsection 3.2.3. Since I am unable to control for their adoption rate over time, this may lead to estimation bias in my results if these apps became more popular over time only in selected markets. However, as mentioned before, it is likely that a government-provided site such as FuelCheck remains the dominant driver of my results due to the credibility and frequency of price updates.

3.4.2 DiD Estimation

My first part of my analysis involves estimating the causal effects of information disclosure on retail price margins. Therefore, for the low frequency margin analysis, I estimate the following difference-in-differences (DiD) regression:

$$\text{Margin}_{it} = \beta_0 + \beta_1 T_i + \beta_2 D_t + \beta_3 (T_i \times D_t) + \kappa_i + \gamma_m + \varepsilon_{it} \tag{3.1}$$

where Margin_{it} is the difference between average monthly retail and wholesale prices in market i in month t (e.g. January 2017). T_i is a dummy variable that equals 1 if market i is in the "treatment group" (NSW) and 0 if market i is in the "control group" (SA, VIC or QLD). D_t is a dummy variable that equals 1 if month t falls in the post-intervention period and equals 0 if month t falls in the pre-intervention period. The interaction term, $T_i \times D_t$ equals 1 if the information disclosure policy is available in city i in month t. In essence, this indicator is equal to 1 for all markets in NSW and 0 for all markets in SA, VIC and QLD from August 2016 due to the simultaneous roll-out of the policy. κ_i's control for time-invariant unobserved heterogeneities in cities. γ_m's control for month-of-year (e.g. January) fixed effects. In some specifications, I also include market $\times$ month-of-year fixed effects ($\kappa_i \times \gamma_m$), to control for seasonal factors in each market. β_3 is the DiD estimator of the policy impact. If the information disclosure policy leads to more competitive markets, then $\beta_3 < 0$. Otherwise, if $\beta_3 > 0$, then margins rise with FuelCheck, raising a concern of potential tacit collusion. ε_{it} is the econometric error of the regression clustered at the state level.

Standard Errors

Statistical inferences rely heavily on how standard errors are computed and using the correct method is imperative for reducing the risk of over-rejecting the null hypothesis. Due to the simultaneous roll-out of the policy that affected all gas stations in the state of NSW, the appropriate method is to cluster standard errors at the state level. This is because clustering standard errors at the market level bears the risk of obtaining incorrectly small standard errors due to the assumption that standard errors within each state are independently distributed. Nevertheless, given that there are only 4 states in my sample, I run into the issue of the presence of small number of clusters.

The traditional cluster bootstrapping technique can work poorly in this case because the size of the bootstrap sample is only 4 and there is only one treated cluster, which means that it is possible for the treatment dummy to be equal to zero for all observations in some bootstrap samples (Bertrand et al., 2004; MacKinnon and Webb, 2017).[12] Therefore, I implement the six-point wild bootstrapping procedure with Webb (2014)'s weights and 9,999 replications (Roodman et al. (2018))[13]

[12]The usual Stata command used, "`bootstrap`", is a nonparametric method that repeatedly resamples observations from the data and calculates a standard error each time.

[13]For this technique, I use Stata's "`boottest`" command. Instead of reporting standard errors, I report p-values because the algorithm does not produce standard errors. Inference is based on p-values and confidence

3.5 Results

This section reports and discusses the results of this essay that investigates the impact of information disclosure on competition.

Figure 3.2 plots the monthly average real margins of markets that are affected by the information disclosure policy and markets that are not affected by the information disclosure policy. Visually, there is a strong co-movement in average margins between markets in NSW and markets in SA, VIC and QLD before the policy was introduced. To further justify that no pre-trend differences exist, I regress margins on the interaction terms between NSW and time dummies that each represents a single month in the pre-intervention period. In addition, it is possible that state-specific effects were driving pre-trend margins. I take this into account by estimating the same regression but I drop one state at a time. If the effects are driven by a specific state, this should be reflected in the estimates that consider the subsample without that state. The results from this exercise are reported in Table 3.4. They suggest that there are no significant pre-trend differences and there are no state-specific effects driving pre-trend margins.

sets and even though it is possible to compute the standard deviation of the bootstrap distribution of the coefficient estimates, this approach relies heavily on the asymptotic normality of the estimates (Roodman et al. (2018)).

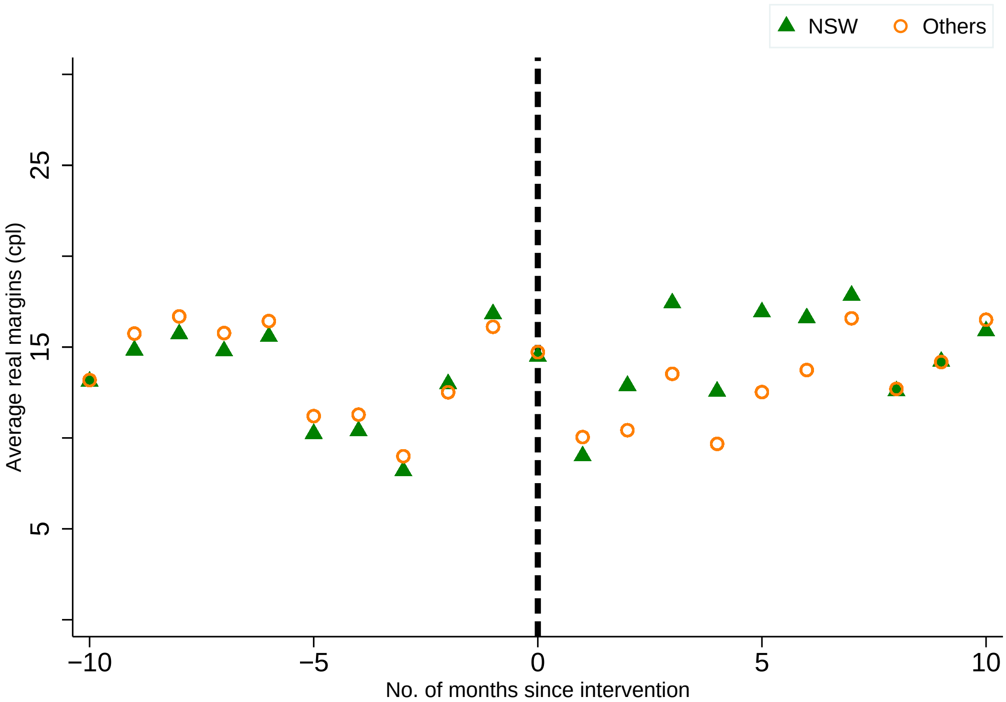


Fig. 3.2 Scatter Plot of Average Real Margins

Returning to Figure 3.2, I find that a break in average real margins is observed 2 months after FuelCheck was launched; average margins in NSW increased above and beyond average margins in SA, VIC and QLD. However, these margin-enhancing effects appear to be temporary and the effects eventually disappear in the long-run.

Motivated by the visual findings, I formally quantify the effects of the policy by running regression 3.1 and report the results in Table 3.5. The regression results reported in columns (1) to (3) show that the policy has led to an increase in average real margins of 1.96 cpl to 2.56 cpl, which translate to an increase in margins of approximately 14.4% to 18.8%, and these estimates are statistically significant. These findings reinforce the patterns observed in Figure 3.2, which suggest that the information disclosure policy has led to margin-enhancing effects.[14] Likewise, Figure 3.3 that reports the time-varying DiD coefficient estimates

[14] Table B.1 in the Appendix also shows that the results are robust to excluding the months when Coles no longer subscribed to Informed Sources' information sharing service.

Table 3.4 Pre-trends Analysis

	Average Real Margins (cpl)			
	(1) excl. SA	(2) excl. VIC	(3) excl. QLD	(4) Pooled
NSW X t=-9	-0.805	-0.833	-0.918	-0.845
	[0.1350]	[0.1185]	[0.1482]	[0.0561]
NSW X t=-8	-0.849	-0.605	-1.297	-0.898
	[0.3698]	[0.1390]	[0.1372]	[0.3228]
NSW X t=-7	-0.921	-0.265	-1.642	-0.918
	[0.4486]	[0.4486]	[0.1969]	[0.3714]
NSW X t=-6	-0.627	-0.468	-1.384	-0.788
	[0.4577]	[0.4105]	[0.1587]	[0.3732]
NSW X t=-5	-0.856	-0.861	-0.993	-0.896
	[0.1529]	[0.1285]	[0.1097]	[0.0507]
NSW X t=-4	-0.821	-0.446	-1.260	-0.827
	[0.3884]	[0.1491]	[0.1182]	[0.3551]
NSW X t=-3	-0.653	0.536	-2.308	-0.745
	[0.6529]	[0.6222]	[0.2841]	[0.6223]
NSW X t=-2	0.0467	0.522	1.266	0.533
	[0.7913]	[0.6157]	[0.3494]	[0.4850]
NSW X t=-1	0.308	1.125	1.091	0.779
	[0.1470]	[0.3693]	[0.3687]	[0.4091]
Time Trend	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
N	820	690	650	930
Adj. R^2	0.695	0.753	0.736	0.729

Notes: The *p*-values associated with the 6-point distribution Wild Bootstrapping procedure using Webb (2014) weights and 9,999 replications are reported in square brackets. Bootstraps are clustered at the state level, which is the level of policy variation. The dependent variable for all specifications is the inflation-adjusted margins (in Australian cents per litre), computed as the difference between average city-level retail price and wholesale TGP in month-year t (e.g. January 2016). Each specification in columns (1)-(3) drops all observations from the specified state. The pooled sample includes 93 markets and the sample period extends from October 2015 to July 2016.

Table 3.5 Effects of Disclosure on Margins

	(1)	(2)	(3)
FuelCheck Disclosure	1.962	1.962	2.560
p-values	[0.0504]	[0.0504]	[0.0601]
Time Trend	Yes	Yes	Yes
Market FE	No	Yes	No
Market X Month-of-Year FE	No	No	Yes
Month-of-Year FE	No	Yes	No
N	1953	1953	1953
Average margin	13.62	13.62	13.62
Effect as % of the mean	14.4%	14.4%	18.8%
Adj. R^2	0.325	0.653	0.620

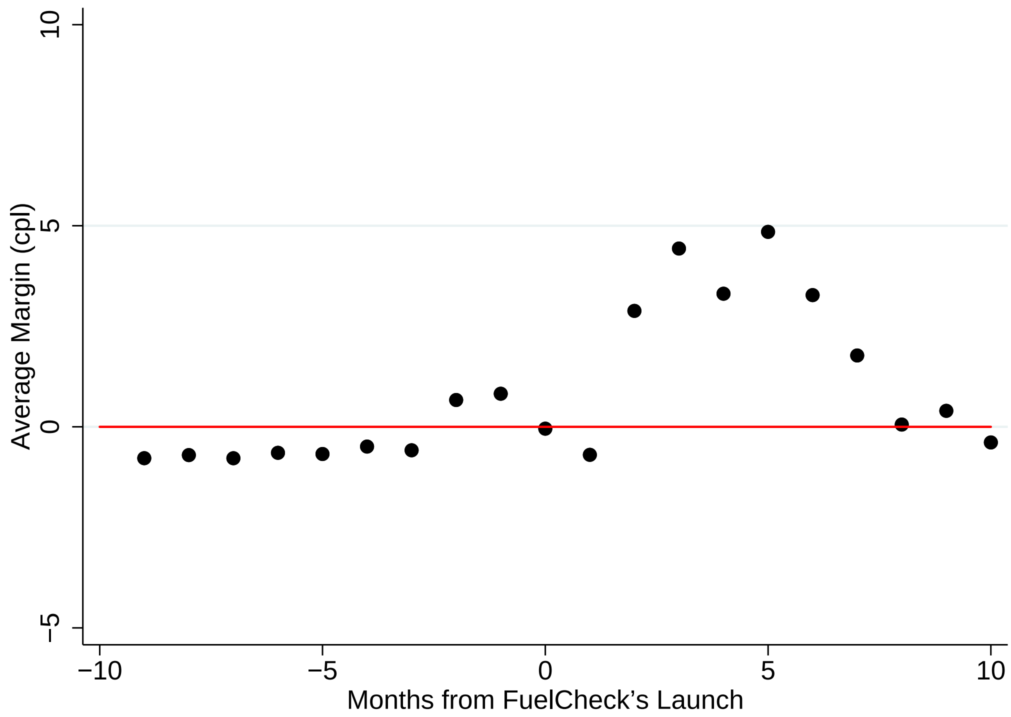
Notes: The p-values associated with the 6-point distribution Wild Bootstrapping procedure using Webb (2014) weights and 9,999 replications are reported in square brackets. Bootstraps are clustered at the state level, which is the level of policy variation. The dependent variable for all specifications is the inflation-adjusted margins (in Australian cents per litre), computed as the difference between average market-level retail price and wholesale TGP in month-year t (e.g. January 2017). The sample includes 93 markets and the sample period extends from October 2015 to June 2017.

from the same regression also shows a compelling increase in margins immediately after FuelCheck was implemented and a decrease in margins in the last 3 months of the sample period. This figure also supports the argument that there are no pre-trend differences in this sample.

3.5.1 Heterogeneous Treatment Effects

Based on these results, I investigate the differential policy effects by capital and regional markets, as well as population size by exploiting big and small markets in the cross-section.

Figure 3.4 plots the average real margins in capital markets on the left panel and regional markets on the right panel. In my sample, Sydney is the state-capital market that is affected by the policy, whereas Adelaide, Melbourne and Brisbane are the state-capital markets that are unaffected by the policy. All other markets in my sample are known as regional markets.



Note: This graph visualises regression coefficient estimates that represent the change in average real margins in month, t, relative to the base month. The base month of this sample is October 2015, which is 10 months before the implementation of the policy ($t = -10$). The regression was estimated on a sample of 93 markets, covering the period from October 2015 to June 2017. I control for market, month-of-year and year fixed effects in the regression estimation.

Fig. 3.3 Time-varying Average Treatment Effects

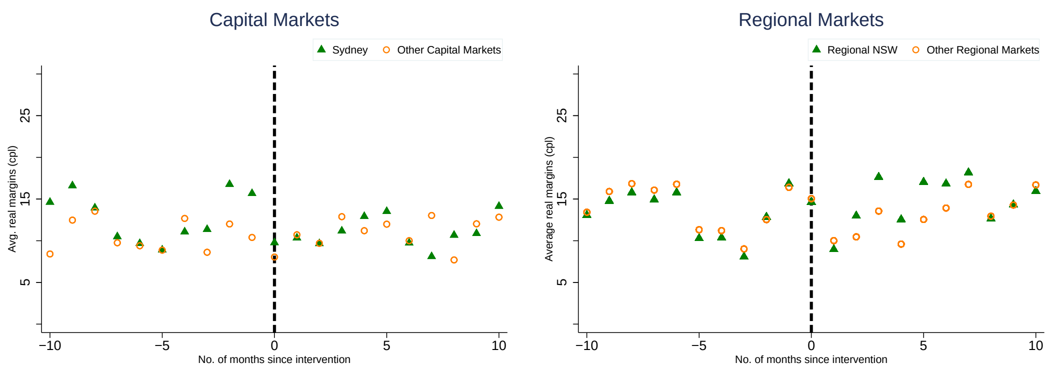


Fig. 3.4 Scatter Plots of Average Real Margins in Capital and Regional Markets

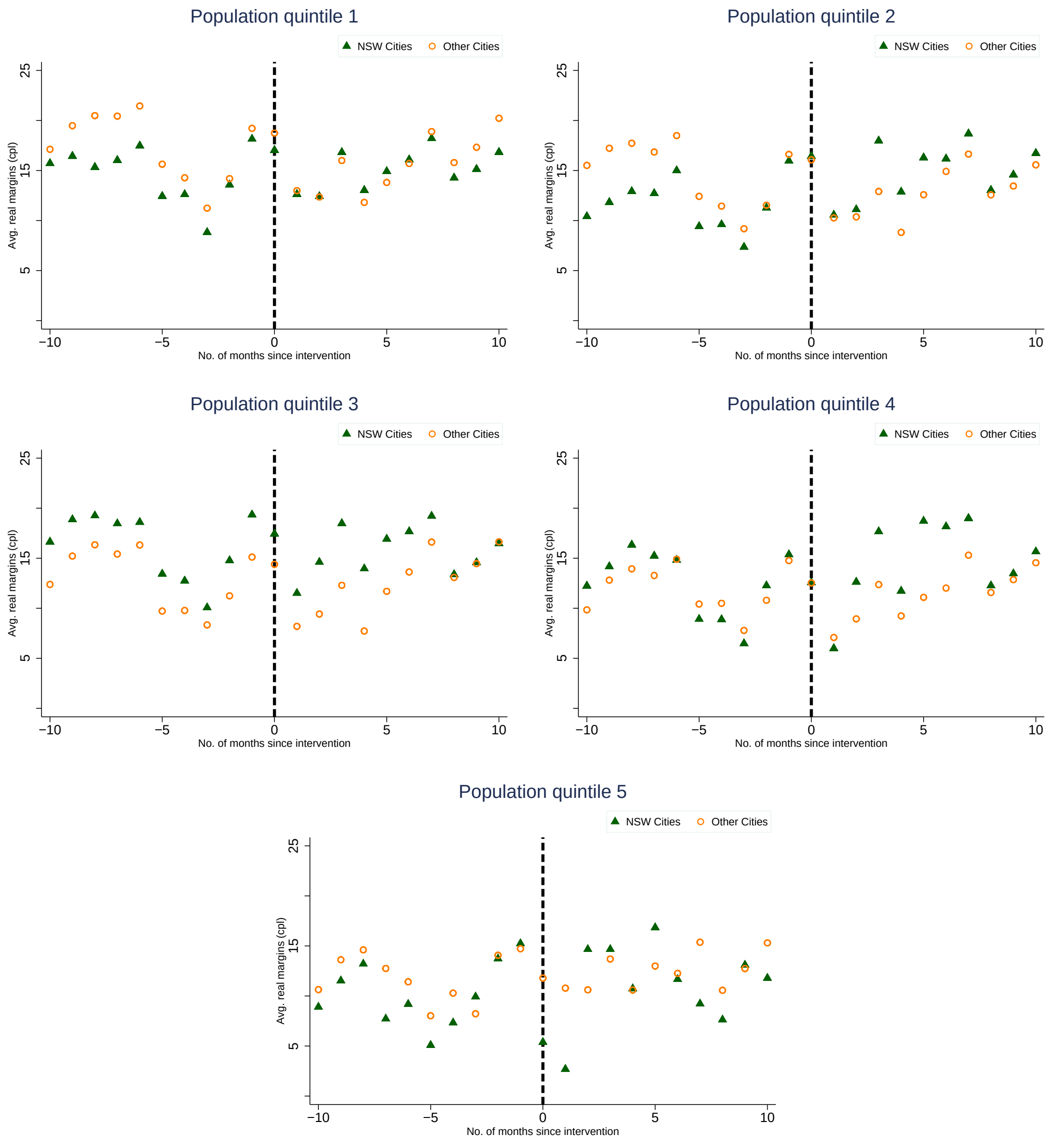
Compared to other capital markets, the policy does not appear to affect margins in Sydney. However, amongst regional markets, the policy appears to have a temporary margin-enhancing effect in NSW's regional markets. This is confirmed by the regression results reported in Table 3.6. The result in column (1) shows that on average, margins decrease by 20.11% in Sydney relative to the capital markets of SA, VIC and QLD, albeit this effect being statistically insignificant. In contrast, the policy leads to a 15.44% increase in average real margins in regional NSW. These results are statistically significant and are robust to controlling for market and year fixed effects.

Table 3.6 Effects of Information Disclosure on Capital and Regional Markets

	Capital Markets	Regional Markets	
	(1)	(2)	(3)
FuelCheck disclosure	-2.224	2.121	2.121
p-values	[0.3364]	[0.0491]	[0.0501]
State FE	Yes	Yes	Yes
Time Trend	Yes	Yes	Yes
Market FE	No	No	Yes
Year FE	No	No	Yes
N	84	1869	1869
Average margin	11.06	13.73	13.73
Effect as % of the mean	-20.11%	15.44%	15.44%
Adj. R^2	0.435	0.267	0.639

Notes: The *p*-values associated with the 6-point distribution Wild Bootstrapping procedure using Webb (2014) weights and 9,999 replications are reported in square brackets. Bootstraps are clustered at the state level, which is the level of policy variation. The dependent variable for all specifications is the inflation-adjusted margins (in Australian cents per litre), computed as the difference between average market-level retail price and wholesale TGP in year-month t (e.g. January 2017). The sample that generates the results in the first column includes the 4 capital markets of NSW, SA, QLD and VIC and the sample that generates the results in the second column includes 89 regional markets of NSW, SA, QLD and VIC. Both sample periods extends from October 2015 to June 2017.

In addition, I also examine whether the policy differentially impacts markets of different population sizes. Figure 3.5 reveals that small to medium-sized regional markets, especially markets in the second and fourth population quintiles, average margins increased immediately after FuelCheck's online platform was launched. There is no clear evidence that margins of markets in the fifth population quintile are affected by the policy.



Note: For context, the population range for markets in population quintile 1 is 275 to 6,071 persons; population quintile 2 is 6,379 to 12,902 persons; population quintile 3 is 12,950 to 19,918; population quintile 4 is 21,505 to 52,075; population quintile 5 is 59,052 to 4,321,53.

Fig. 3.5 Scatter Plots of Average Real Margins, by Population Quintiles

The graphical findings are reaffirmed by results from DiD regressions reported in Table 3.7. Column (1) shows that the size of increase in margins due to the policy decreases with population size but it is statistically insignificant. Column (2) shows that the policy increased margins of markets in the 1st, 2nd, 3rd and 4th population quintiles, even though they are only statistically significant for markets in the 2nd and 4th population quintiles. This exercise shows that the policy potentially leads to more margin-enhancing outcomes in small to medium-sized regional markets than in large markets.[15]

Table 3.7 Effects of Disclosure on Margins, by Population Size

	(1)	(2)
FuelCheck disclosure	2.174	-0.015
	[0.0519]	[0.8724]
Disclosure X Population ('0000s)	-0.0153	
	[0.2350]	
Disclosure X Population quintile 1		2.110
		[0.2629]
Disclosure X Population quintile 2		4.910
		[0.0927]
Disclosure X Population quintile 3		0.0645
		[0.8913]
Disclosure X Population quintile 4		2.224
		[0.0677]
State FE	Yes	Yes
Time Trend	Yes	Yes
Population Quintile FE	Yes	Yes
State X Population Quintile FE	Yes	Yes
Population Quintile Trend	Yes	Yes
Month-of-Year FE	Yes	Yes
N	1953	1953
Adj. R^2	0.107	0.196

Notes: The p-values associated with the 6-point distribution Wild Bootstrapping procedure using Webb(2014) weights and 9999 replications are reported in parentheses. Bootstraps are clustered at the state level, which is the level of policy variation. The dependent variable for all specifications is the inflation-adjusted margins (in Australian cents per litre), computed as the difference between average market-level retail price and wholesale TGP in year-month t (e.g. January 2017). The sample that generates the results in all columns includes 93 markets in NSW, SA, QLD and VIC and the sample period extends from October 2015 to June 2017.

[15] Despite showing differences in the magnitude of policy effects across population quintiles, a joint hypothesis test shows that none of the estimates are significantly different from each other.

3.5.2 Within-Market Policy Impacts

Digging deeper, I investigate the time-varying treatment effects of the policy for each market in NSW by re-running the regression from equation 3.1, interacting the number of months before and after FuelCheck was introduced with each market in NSW. The regression is now:

$$\text{Margin}_{it} = \beta_0 + \sum_i \beta_{1i} T_i + \sum_{t=-9}^{10} \beta_{2t} t + \sum_i \sum_{t=-9}^{10} \beta_{3it} (T_i \times t) + \varepsilon_{it} \tag{3.2}$$

Figure 3.6 plots the within-market time-varying impacts of the policy in market i and month t, β_{3it} coefficient estimates.[16] At a glance, the policy effects across markets are mixed. For example, in markets such as Hay and Narrabri, the policy does not appear to have any effect on margins. In Casino and Dubbo, the policy appears to cause a short-run increase in margins, whereas in Forbes and Wagga Wagga, the effects appear to be more permanent.[17] I discuss some of the possible reasons for the short-run and long-run policy effects in the next section using real-time station-level price updates from FuelCheck.

In sum, the information disclosure policy introduced in NSW leads to margin-enhancing effects in regional markets. I do not find, however, any evidence of margin-enhancing effects of the policy in the capital of NSW, Sydney. One possible explanation is that local markets in Sydney are more competitive because demand elasticities are higher. An alternative explanation relates to the proportion of sellers that are already informed through Informed Sources' information-sharing service. In Sydney, approximately 65% of stations already had access to their rivals' prices through Informed Sources' service, compared to only 56% of stations in regional markets.[18] If information disclosure facilitates coordination on higher prices, then markets that are exposed to such information for the first time are more

[16] The markets in Figure 3.6 are ordered by population quintiles.

[17] It is difficult to empirically identify which markets actually experience margin increases. This is because the policy was introduced at the state level, which limits the number of clusters for any standard error calculations.

[18] These percentages are calculated based on the assumption that all stations trading under one of the following brands – BP, Caltex, Coles, Woolworths and 7-Eleven – subscribe to Informed Sources' service (see Table B.2

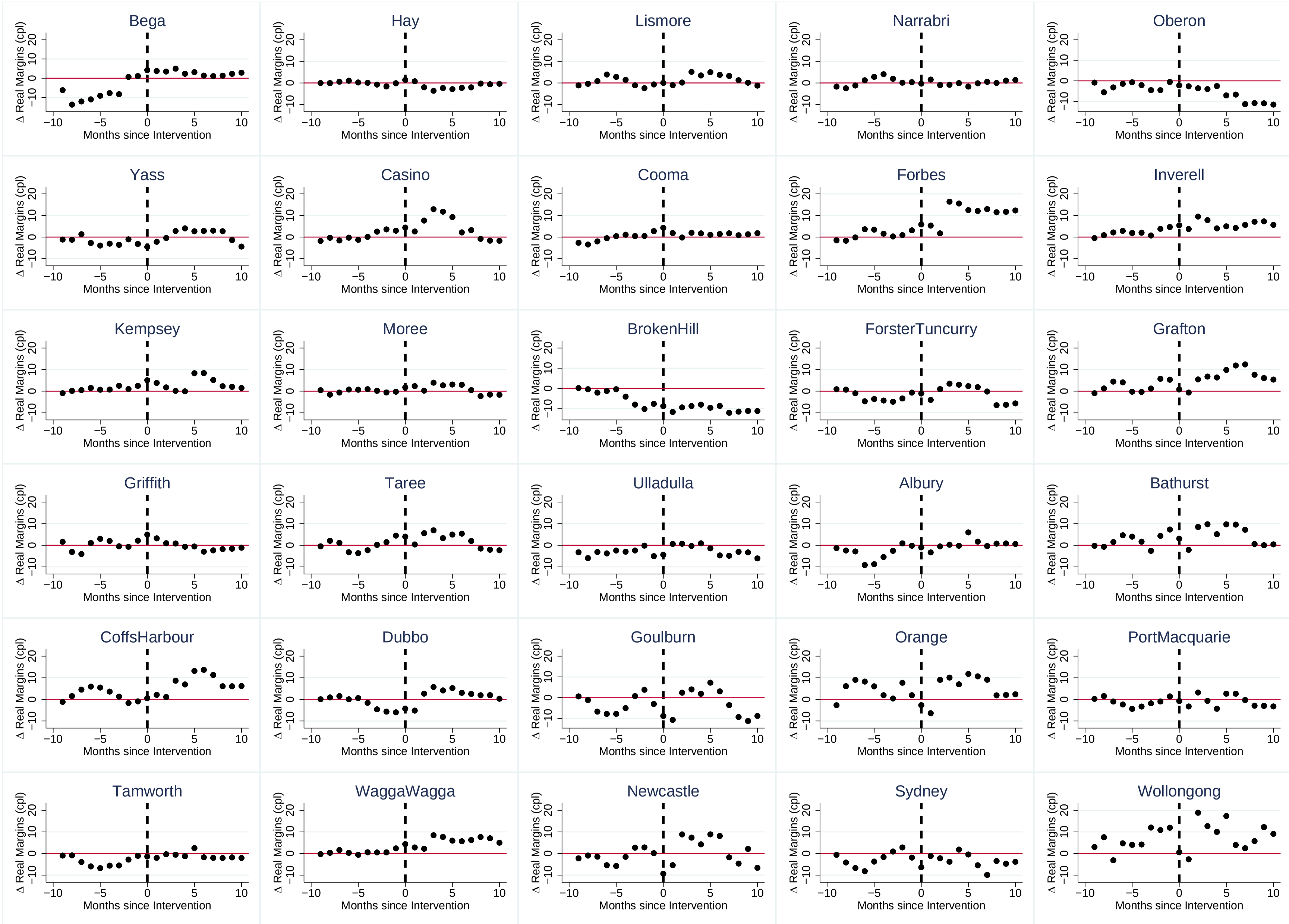


Fig. 3.6 Time-varying DiD Estimates in regional markets of NSW

susceptible to margin increases. Therefore, it is possible that FuelCheck's policy leads to more margin-enhancing effects in regional markets because information disclosure is new to most stations in those markets.

3.5.3 How does Information Disclosure Facilitate Margin Increases?

So far, the results have suggested that the introduction of information disclosure policy in NSW leads to margin-enhancing outcomes in small regional markets. While these results highlight important policy concerns in regional markets, it is not clear at this stage if the margin increases are driven by tacit collusion or other changes in the market. Therefore, in the next part of my analysis, I conduct a high frequency price analysis using real-time price updates to investigate what drives margin increases.

3.5.4 Coles Coordinates Equilibrium Transition in Retail Prices

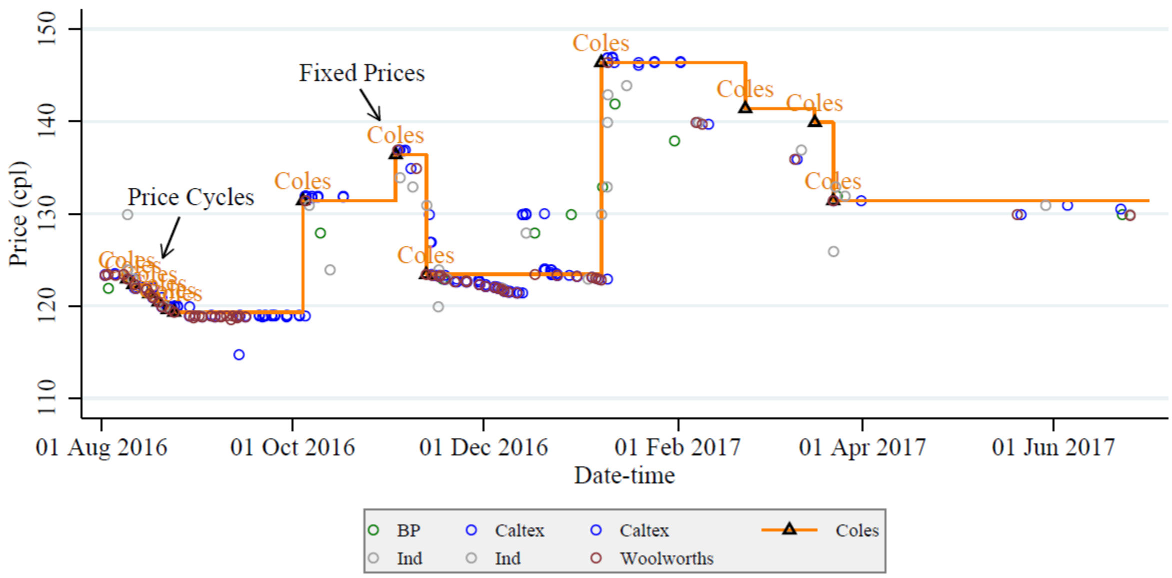
Figure 3.7 plots real-time station-level price changes in Port Macquarie, NSW. It highlights the degree to which gas stations updated their prices changed dramatically between August 2016 to June 2017. In the period between August and October 2016, gas stations were aggressively undercutting each others' prices until they arrived at a war of attrition and this lasted until Coles decided to relent. These features resemble Edgeworth price cycles[19] that are commonly found in many retail gasoline markets.[20]

A noticeable change occurred in September 2016, when Coles stopped undercutting their rivals' prices. Instead, their prices remained stable even when their rivals continued undercutting one another. By doing this, Coles was potentially sacrificing profits in order to

for station tally by brand). However, these numbers are only estimates as I do not have access to the list of Informed Sources' subscribers.

[19]In Maskin and Tirole (1988)'s model, the prices that each firm can choose are restricted to a finite grid. The size of the grid is k. They interpret k as the smallest amount by which a firm can undercut, which in most cases here is a fraction of a cent.

[20]For instance, price cycles are also documented in retail gasoline markets in the United States (Lewis, 2012), Canada (Noel, 2007) and Australia (Wang, 2009).



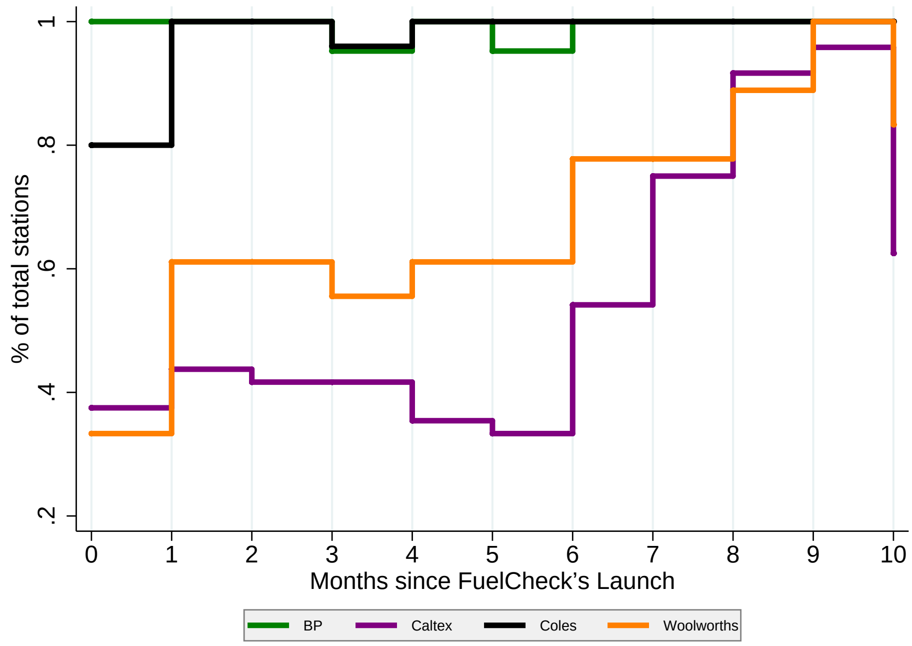
Note: This graph plots the universe of station-level price updates in Port Macquarie, NSW. Markers are colour-coded by brand. Station brands other than BP, Caltex, Coles and Woolworths are labeled as "Ind". The sample period is from August 1, 2016 to June 30, 2017.

Fig. 3.7 Coles' Transition to A Fixed Price Equilibrium

communicate their intention to transit to a fixed price regime. By January 2017, however, the other gas stations also became more stable compared to the period before January 2017. This reveals a key result in this essay: *Coles coordinated an equilibrium transition from a price cycle regime to a fixed price regime immediately after the introduction of FuelCheck.*

The scatter plots of price changes for other regional markets in NSW are presented in Appendix B.1–B.20. Most markets appear to demonstrate a similar transition in pricing regime initiated by Coles except for markets in the top population quintile – Newcastle, Sydney and Wollongong.[21] I find that neither Coles nor any other brand initiated any transition in pricing regimes in these large markets. Instead, firms consistently engaged in price cycles throughout the sample period of this study.

[21]The scatter plots of station-level price updates for these markets are not presented in this essay due to the difficulty of presenting a large volume of prices given the large number of gas stations in these markets.



Note: This graph illustrates the equilibrium transition from a price cycle regime to a fixed price regime. It plots the share of stations of a given brand that engage in fewer than 5 price updates in a given month. This sample consists of 27 regional markets in New South Wales that are ranked in the 1st to 4th population quintiles. This sample spans the period from August 2016 – June 2017.

Fig. 3.8 Share of Stations in Fixed Price Regime by Market

I further investigate this state-wide equilibrium transition across small regional markets. Figure 3.8 demonstrates how BP, Caltex, Coles and Woolworths transition from a price cycle regime to a fixed price regime. To illustrate this transition, I define stations as "cycling stations" and "fixed price stations" based on the frequency of price changes in a given month.[22] A station is defined as a "cycling station" in a given month if the station updates prices at least 5 times in that month.[23] Otherwise, the station is defined as a "fixed-price" station. The vertical axis contains the percentage of total BP, Caltex, Coles and Woolworths branded stations that adopt a fixed price regime and the horizontal axis contains the elapsed months from FuelCheck's launch in August 2016.

[22]Consecutive price uploads of the same price are not counted as price changes.
[23]The results are robust to 4 and 6 price updates per month.

This figure shows Coles' role in leading the transition from a price cycle equilibrium to a fixed price equilibrium. When FuelCheck was first launched in August 2016, only 40% of Caltex and Woolworths branded stations adopted a fixed price regime, compared to 80% of Coles stations and 100% of BP stations.[24] However, the percentage of Caltex and Woolworths branded stations began to increase over time. By May 2017, most branded stations in regional NSW were in a fixed price regime.

3.5.5 Discussion

The graphical findings beg the following questions: Why do firms transition to a fixed price equilibrium and how does information disclosure help them achieve it?

Before diving into the discussions, it is important to note that I cannot claim that information disclosure *causes* or *starts* collusion. As the literature is still divided on whether price cycles are competitive or collusive, it is possible that firms were already tacitly colluding even before the policy was introduced.[25] Therefore, I am unable to determine if firms were already tacitly colluding even in the absence of information disclosure. Instead, I am arguing that information disclosure plays an important role in establishing a transition to a more profitable equilibrium pricing regime.

Retailer Size Asymmetry

Since the transition in pricing equilibrium found in the high frequency price analysis *coincides* with margin increases documented in the low frequency margin analysis, I conclude that

[24] Although this graph shows that BP is also a price leader, the scatter plots of price updates show that Coles was actually the one leading the transitions.

[25] For instance, Foros and Steen (2008), Clark and Houde (2013) and Byrne and De Roos (2019) find evidence of collusive price cycles in Norway, Canada and Perth, Australia, by discovering that the decision to relent prices is based on focal prices and day-of-week instead of price levels. These contrast Maskin and Tirole (1988)'s theory that suggests firms only choose to relent when prices approach marginal costs. Nevertheless, the results currently favour the conclusion that price cycles are indicative of stronger competition and are a source of lower prices for consumers (Noel et al., 2011).

firms are better off under a fixed price regime compared to a price cycle regime.[26] However, it is unclear as to why the transition to a fixed price regime only occurred in smaller markets and not larger markets. The closest theory that offers a potential explanation for this is Eckert (2003)'s model. His model extends Maskin and Tirole (1988)'s model that suggests both constant price and price cycle equilibria exist by demonstrating that a constant price equilibrium cannot possibly exist in the presence of sufficiently small retailers. This is because a small retailer will never mix between setting a high and a low price in the event that they observe their rival set a low price, which is necessary for ensuring that there is a positive probability of restoring the focal price or monopoly price.[27] Therefore, markets with highly divisive market shares between retailers can only sustain a price cycle equilibrium, which potentially explains why a fixed price equilibrium is not observed in the major markets in my sample.

Because the literature does not suggest a standard measure of the presence of independents or small chains, I follow one of Eckert (2003)'s methods to capture the presence of small marketers. That is, I compute the percentage of stations in a market operated by chains with less than 5% of the total stations in the market.[28] On average, 19.6% of stations in markets in the 5th population quintile are operated by small chains. In contrast, only 5.4% of stations in markets in the 1st to 4th population quintiles are operated by small chains.[29]

[26]One of the caveats in this essay is the unavailability of volume data to compute volume-weighted margins. If consumers can time purchases, they would make most of their purchases on the lowest-priced day of the cycle (Noel, 2018). Therefore, the policy effects documented in this essay only represents the lower bound of the impact and margins will likely be even higher if volume-weighted margins are assessed instead.

[27]In their paper, firm size is determined by the number of stations owned by a firm.

[28]In his paper, he also uses another measure, which is the fraction of stations in a market operated under a refinery brand or a second brand. However, the concern with this method is that an independent chain could in fact be larger than chains operated by refiners. For example, 7-Eleven and Metro, both non-refiner marketers or supermarkets in my sample, operate more stations than BP and Woolworths in the large markets. For this reason, I choose the alternative measure as my preferred method of measuring the presence of independent brands.

[29]The 5% threshold is arbitrary and using other thresholds yield similar results. For instance, 24.9% of stations in markets in the 5th population quintile and 11.7% of stations in markets in the 1st to 4th population quintiles are operated by small chains when a 7% threshold is used; 42% of stations in markets in the 5th population quintile and 25.6% of stations in markets in the 1st to 4th population quintiles are operated by small chains when a 10% threshold is used.

These demonstrate a stronger presence of asymmetric retailers in the large markets, which explains why price cycles and not fixed prices exist in major markets.

Feasibility of Fixed Price Equilibrium under Information Disclosure

What is the rationale behind the equilibrium transition from price cycles to fixed prices? There are a few reasons why firms prefer coordinating on fixed prices over price cycles. For instance, if firms can successfully coordinate on the monopoly price, profits are maximised in the long-run and margins are stable. On the other hand, the monopoly price is at best only attained for a short period of time under a price cycle regime. For the remaining days, firms go through a period of low prices and hence margins during the undercutting phase of the cycle until prices reach marginal costs and one firm decides to relent. In addition, the lack of predictable cycles may also enhance margins as consumers can no longer benefit from timing their purchases on low-priced days of the cycle (Noel, 2018). Therefore, if these are true, then why were firms engaging in price cycles before FuelCheck was introduced?

In the absence of FuelCheck, large firms use their market power to coordinate a margin increase with small firms through large, infrequent price jumps. Unlike large firms, small firms did not subscribe to Informed Sources' price-sharing service. Price cycles were used as a coordination tool instead because firms were unable to establish a credible punishment threat, which is needed if firms want to successfully coordinate on a fixed price. This is because it is difficult for firms to prove guilt or assign blame and Genesove and Mullin (2001) suggest two reasons for this. First, there may have been no intention to break the agreement and the action could have just been an "honest mistake". Second, a firm needs to garner support from other firms before accusing another firm of cheating. If the accusing firm acts alone, it may stand accused of cheating itself and may trigger retaliation against them. However, the threat of punishment becomes more credible under FuelCheck. This is because FuelCheck creates a paper trail of information, which enables it to act as a medium to identify and detect the perpetrator. This enables firms in the cartel to credibly enforce incentive compatibility constraints.

Another reason why FuelCheck is crucial for fixed price coordination is that it increases the speed of detecting any deviation from the equilibrium path. Before FuelCheck was introduced, deviations would have taken longer to detect because station owners would only be able to observe their rivals' prices by driving to their stations. Since detection was not instant, large firms would risk losing market share if a small firm had undercut their prices. Now, because the duration a small firm who deviates can enjoy higher profits before they are discovered decreases, the incentive for small firms to cheat is smaller (Luco, 2018).

Furthermore, the ability of small firms to observe prices of large firms under this policy is also important for fixed price coordination. FuelCheck provides small firms access to historical and real-time price information of the large firms. With these information, small firms can develop a better understanding of price matching mechanisms, such as what the coordinated price is and the punishment payoffs if they deviate.

3.5.6 Long-run Effects

In the final month of my sample period, June 2017, margins collapsed and fell back to pre-FuelCheck margins in some but not all markets. It appears that the transition to a fixed price equilibrium is permanent in some markets, but only temporary in others.

Therefore, I attempt to explain some of these differences by associating the long-term impact of this policy with a few market characteristics. The within-market effects of the policy in June 2017 are represented by the $\beta_{3i,t=10}$ coefficient estimates from regression 3.2. Figure 3.9 illustrates the association between the policy's long-run effects against the share of independent stations, population per station and population size. I expect that markets with a larger presence of independent stations, larger customer base per station and larger population size are less likely to accomplish a permanent transition to a fixed price equilibrium because the incentives to deviate and undercut branded firms are higher in such markets. Nevertheless, the top left panel of the figure shows that there is at best a weak negative relationship between the share of independent stations and the long-run average treatment effects of the policy. A weak negative relationship is also observed between the average treatment effect and the average population size per station. Population size does not appear to be related to the long-run policy effects. These suggest that the stability of the fixed price equilibrium is potentially driven by other sources of firm heterogeneities.

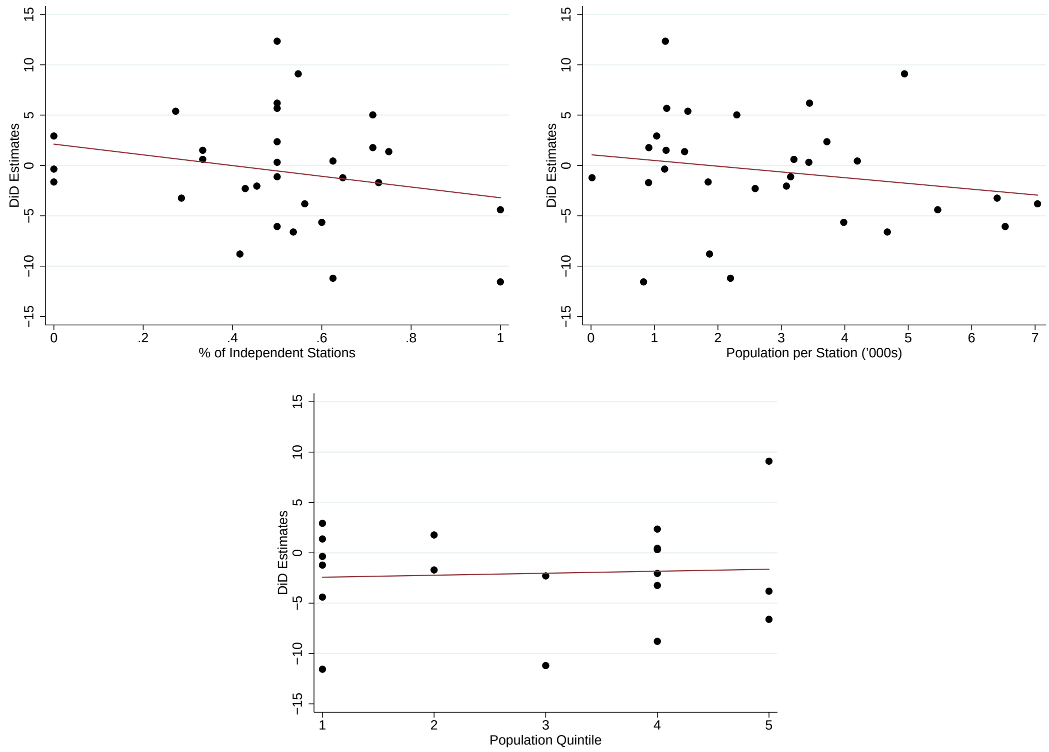


Fig. 3.9 How Long-run Effects of Information Disclosure Relate to Market Characteristics

There are other reasons that can potentially explain the stability of the fixed price equilibrium. In a dynamic rivalry, collusion can only be successful when players understand the rules of the game, especially what constitutes a punishment and when they are being punished (Slade, 1992).[30] However, when players cannot overtly discuss their strategies,

[30]Stigler (1964) argues that collusion is difficult to sustain due to an incentive to defect from an agreed price that is above marginal cost. The cartel problem thus consists of reaching an agreement, detecting secret price cuts, and punishing the cheater.

which is the case here, establishing these rules becomes difficult. Furthermore, perhaps some firms are taking a longer time to learn how to coordinate. For instance, Byrne and De Roos (2019) show that it took gas stations in Perth, Australia three and a half years to transition to a more profit-enhancing equilibrium. Alternatively, unobserved demand shocks to the econometrician could have affected the stability of the price equilibrium.

3.6 Conclusion

This essay investigates the effects of an information disclosure policy on competition in an oligopoly market. The policy provides consumers access to real-time information of gas prices in the state of New South Wales, Australia. Price reporting is mandatory under this policy and is effective from August 1, 2016.

Information disclosure policies reduce search frictions, increase demand elasticities and should create competition. However, if they also make it easier for firms to monitor their rivals' prices, then these raise concerns about tacit collusion. The equilibrium impact depends on which effect dominates. To explore this issue, I estimate the causal impact of a price transparency platform for retail gasoline known as FuelCheck, on retail margins. For this purpose, I exploit data from a large cross-section of markets that are affected and unaffected by the policy, pre- and post-intervention. My findings show that the policy has led to margin-enhancing effects in small regional markets.

Next, using real-time firm-level data, I reveal a coordinated transition to a more profitable price equilibrium *immediately* after the policy was introduced. In particular, Coles, one of the largest players in the market, used prices to communicate their intention to transit from a price cycle equilibrium to a fixed price equilibrium. Their coordination strategy was a success, as other firms soon began to engage in more stable prices too.

In repeated price-setting games, pricing strategies that resemble collusion are bound to be exercised even without direct communication. However, there is little evidence on tacit collusion despite its importance for antitrust policy. Therefore, my study contributes to this area by showing new evidence of price leadership in tacit coordination.

Moreover, my results that show that information disclosure has potential anti-competitive effects in small regional markets may have distributional consequences. Low-income consumers who are less price-sensitive are likely to be affected the most. Furthermore, even if consumers in small cities become perfectly aware, the gains from search are smaller in these cities as there are fewer stations to choose from. In such markets, the gains of information disclosure for producers are likely to exceed the gains for consumers. Therefore, policy-makers should consider winners and losers in each region before implementing "blanket policies" that affects all markets.

Finally, this essay underscores the use of high frequency data to reveal tacit collusion practices that can be valuable for future antitrust cases. The completeness of station-level data has led to the discovery of the transition in price equilibria. There are future plans to continue collecting data beyond the completion of this thesis to investigate the transition of price equilibria in the long-run.

Chapter 4

Retail Search and Socio-economic Disadvantage in Retail Gasoline Markets

4.1 Introduction

Before the emergence of online platforms, searching for deals was both time-consuming and confusing. Now, there are thousands of price comparison websites that dramatically lower the cost of comparing prices of similar products across companies. Examples abound, including uSwitch (energy), Expedia (tourism), Confused.com (car insurance), and others. Yet, in many homogeneous product markets, price dispersion still exists. Persistent price dispersion may lead to even more buyer confusion and misguided purchase decisions leading to, for example, less informed consumers paying higher prices.

Since Varian (1980)'s model was introduced, there has been an unprecedented growth in studies on the equilibrium relationship between search and price dispersion.[1] In spite of this, few studies have studied consumer search dynamics using direct search methods.[2]

[1] See Baye et al. (2006) for a comprehensive review of the search literature.

[2] Exceptions include Byrne and de Roos (2017), who access daily aggregate website visits to a fuel price transparency platform in Perth, Australia and Blake et al. (2016) who obtain data on consumer search activity on eBay.

This is unsurprising, mainly because search data are usually proprietary-owned and are generally unavailable. Consequently, early research in this area rely on indirect search measures because actual search activities are not observed. For instance, Sorensen (2000) finds less variation in prices of repeat medical prescriptions compared to one-time purchases. He explains that customers who are given repeat prescriptions have higher incentives to search for the cheapest price given that they will be purchasing the same product multiple times. Brown and Goolsbee (2002) use internet adoption rate as a proxy for comparing life insurance policies on an online platform and find that price dispersion decreased when more and more consumers started searching online. In the context of retail gasoline, Chandra and Tappata (2011) indirectly estimate temporal search costs by observing variations in price rankings over time. They find that rank reversals and prices spreads among gas station pairs that are not located at the same street intersection are consistently higher than those located at the same intersection, which suggests that price dispersion is higher when search cost is higher.

There is a wealth of benefits from using direct search measures in applications of search models. The ability to observe how consumers *actually* search not only informs modelling assumptions, but it also potentially reveals behavioural insights that existing search models fail to capture. For instance, existing models assume that consumers are rational, but in reality, consumers are limited in their ability to gather and process information and this may lead to previously-unexplained differences in search behaviour across households from different socio-economic backgrounds. Such heterogeneity is important because it potentially gives rise to heterogeneous impacts of price transparency policies across different groups of consumers.

I establish the relationship between search and price dispersion across socio-economic groups among adopters of an online search platform, by constructing a direct measure of search intensity using a unique set of data from a price transparency website. Furthermore, I gain access to the universe of prices across 186 markets for over six months from the platform. Combined, these data allow me to explore search responses to aggregate and local changes in market outcomes. Using disaggregated data has important implications on the interpretation of results. For instance, a study by Levin et al. (2017) shows that using disaggregated data yield significantly different demand elasticities for retail gasoline than from using aggregated data. Therefore, in my analysis, the granularity of my data enables me to study important heterogeneities that are often masked in aggregated data. Moreover, there is high variation in income and socio-economic status across the markets in my data that I can exploit to study how search responses differ across consumer demographics.

In addition, the richness of the data also enables me to explore different mechanisms that affect search responses. Like Byrne and de Roos (2017), I explore the extent to which cross-sectional and intertemporal incentives influence consumer search decisions. Isolating these mechanisms has several benefits. First, it addresses the question of whether consumers make myopic or dynamic search decisions. Since most competition models are currently built on the assumption that consumers are myopic, they could produce inaccurate predictions of policy impacts if consumers respond to dynamic incentives instead. In addition, studying how these mechanisms vary across socio-economic groups . For instance, if low-income platform users are less aware of intertemporal gains, then policies that are directly targeted at helping them search over time can be beneficial.

The context of my study is the retail gasoline market of Greater Sydney, Australia. It befits the purpose of this study for the following reasons. First, the existence of price cycles in most Australian urban gasoline markets facilitates my ability to empirically document search behaviour in the presence of supply-side shocks, such as sudden and large price jumps. The timing of these jumps is difficult to predict and creates a layer of complexity for timing gasoline purchases (de Roos and Smirnov, 2017). Implicit to these “shocks” are also cross-sectional search incentives that arise from the failure of gas stations to coordinate price increases. This, combined with the time-varying aspect of the price cycle enable me to disentangle cross-sectional and intertemporal search behaviour.

Second, the product is homogeneous, which allows me to abstract product differentiation from consumers' search decisions, except for spatial differentiation. Finally, analysing consumer search on retail gasoline is a policy relevant topic as consumers often spend a significant portion of their disposable income at gasoline stations. An average Australian household spends 4.5% of their weekly income on motor vehicle fuel, and this compares to 23.4% of their income being spent on food and 27.7% on dwelling costs. For this and other reasons (such as price competition and tacit collusion), the sector is frequently subject to inquiries from competition authorities. While understanding the welfare effects around price cycles is fundamental from a policy perspective, this aspect has received limited attention from authorities and researchers so far.

For my analysis, I adopt Byrne and de Roos (2017)'s model of search and price dispersion. To my knowledge, it is the only paper so far that considers dynamic, as well as cross-sectional search responses. However, they use city-level data of Perth, Australia, which inherently assumes that search elasticities are constant throughout the city of 1.7 million people. I improve on their paper by also measuring search responses to changes in price dispersion at the neighbourhood level. My model unfolds in two parts. In the first part, I present a baseline regression model that estimates average search reaction to supply-side price shocks generated by price cycles across different socio-economic groups. Then, I extend this model by adding price dispersion and changes in price levels in order to estimate the relative importance of cross-sectional and intertemporal search incentives.[3]

My baseline results highlight large heterogeneities in search responses to price shocks across socio-economic groups, creating a "search reaction gap" between socio-economic

[3]It is important to acknowledge the potential simultaneity of search and price dispersion in this market. However, since search on the platform is still low and there is evidence of gas stations in Greater Sydney coordinating on price cycles for many years, it is unlikely that retailers are responding to search on the platform at this stage.

groups. Specifically, the search platform adopters in the lowest socio-economic group engage the least in search during price jumps and their reactions are delayed compared to users from higher socio-economic groups. This is concerning, since it suggests that the most vulnerable consumers appear to be the least sophisticated at searching, which makes them less informed.

Price comparison websites are generally favourable, but the distribution of benefits from these services are not well-known. It involves developing an understanding of who engages with these platforms and when they use it, which to the best of my knowledge, is scarce in the literature. Gaining empirical insights on this matter is essential for policy design especially if policy-makers are concerned about targeting vulnerable consumers. My findings reveal how inattention hinders certain groups of users from engaging effectively with the platform, hence policies should be tailored to increase competition through consumer participation and awareness.[4] In my case study, applying soft nudges such as sending low-income consumers notifications on the best time to purchase fuel yield the most bang for the buck.

The rest of this chapter is structured as follows. Section 4.2 provides an overview of the retail gasoline industry. Section 4.3 describes the data used and Sections 4.4 and 4.5 discuss the empirical model and findings of my analysis. Section 4.6 concludes.

4.2 Industry

The Australian gasoline industry operates at three levels: refining and importing, wholesale, and retail. About 60% of unleaded petrol sold in Australia is refined by local refineries, while the remainder is imported. As a significant importer of gasoline from the Asia Pacific region, wholesale prices are benchmarked against Singapore's gasoline prices (MOGAS 95). Like most OECD nations, the refining, importing and wholesale sectors in Australia are highly concentrated and major oil companies – BP, Caltex, Mobil and Shell – dominate upstream

[4] This is supported by results from a recent study in the mortgage industry by Allen et al. (2014). They find that heterogeneity in mortgage rates are largely explained by search and negotiation abilities and that increasing competition does not affect those at the higher end of the price distribution.

petroleum. They also influence retail prices across their retail gasoline station networks through vertically-integrated contracts or franchising agreements.

My research focuses on the retail gasoline market of Greater Sydney, the largest city in Australia with a population size of 5.5 million people. The key players at the retail level are oil majors (market shares in parentheses) – BP (19%) and Caltex (17%) – and supermarkets – Coles (19%) and Woolworths (18%). Both Coles and Woolworths have a major presence in Australia's retail grocery sector and have formed exclusive alliances with Shell and Caltex for their retail gasoline businesses ACCC (2018d). They also compete on prices via docket discounts from purchasing groceries at their stores. The remaining 27% of the market share is split amongst independent retailers.

4.2.1 Retail Prices

In this setting, retail prices are set locally by gas stations. For years, the ACCC has documented price cycles in Greater Sydney, Australia. These cycles are characterised by sudden and infrequent price jumps, followed by a period of slow price undercutting. Price cycles are common in retail gasoline markets worldwide and they emulate the Edgeworth price cycles theorised by Maskin and Tirole (1988). There are many studies that support the Edgeworth price cycle theory, including Eckert (2003), Noel (2007), Atkinson (2009), Wang (2009), Lewis (2012), and others.

The following figures provide visual representations of price cycles and some of their features. Figure 4.1 illustrates aggregate retail gasoline price cycles in Greater Sydney, Australia throughout my sample period from September 17, 2016[5] to March 8, 2017.[6] The cyclical patterns observed in retail prices directly contrast the movements in wholesale prices (TGP), hence ruling out any cost-based explanations for the pricing patterns observed. Figure

[5]Despite the availability of price data from August 1, 2016, I only plot prices for the duration that search data are also available.

[6]The data sources from which I obtain these prices are described in Section 4.2.2.

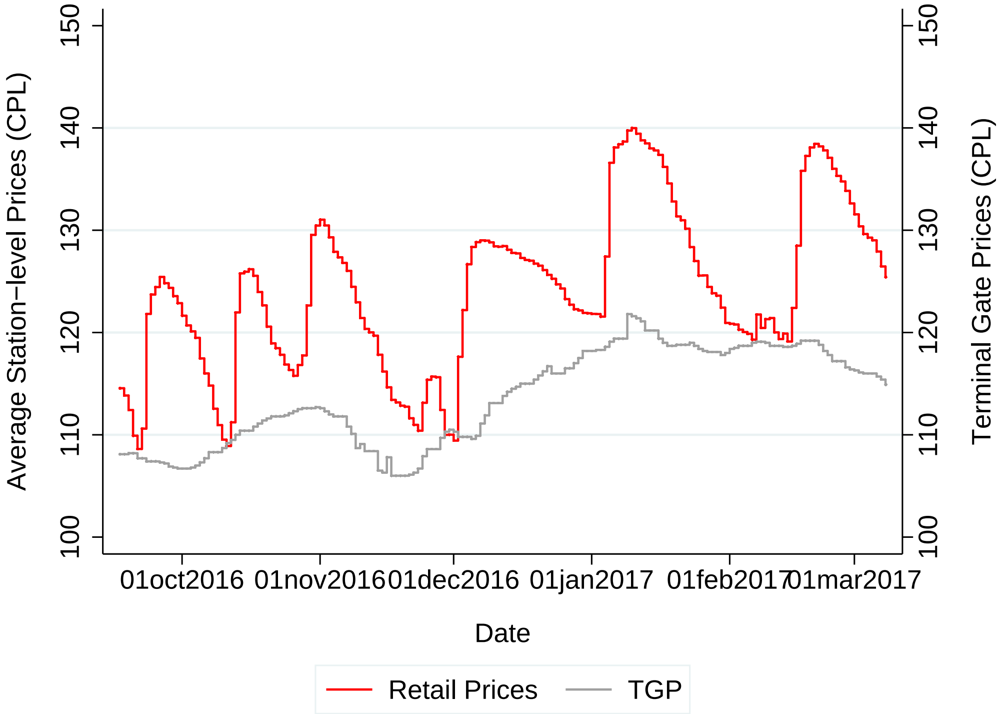


Fig. 4.1 Aggregate Price Cycles

4.2 shows that price dispersion, measured by the standard deviation of prices, also fluctuates according to price cycles, where large variations in prices are observed during the upward relenting phase of the price cycle and low variations at other times.

In addition, price jumps appear to be coordinated. This is presented in Figure 4.3, which plots the average station-level prices in Greater Sydney by the four major retailer brands and an independent brand. Visually, there are six price cycles in the sample period, and each cycle lasts for approximately one month.[7] At the start of each cycle, stations engage in price jumps of approximately 25-30 cpl above the average price of the previous cycle's trough within 2-3 days.

This figure highlights two important features for my study. First, price jumps are not perfectly coordinated, giving rise to cross-sectional price dispersion when some, but not all,

[7] These exclude two potential "false starts", where a few stations attempted to raise prices but quickly reversed their decision when they realised that other stations were not following their lead.

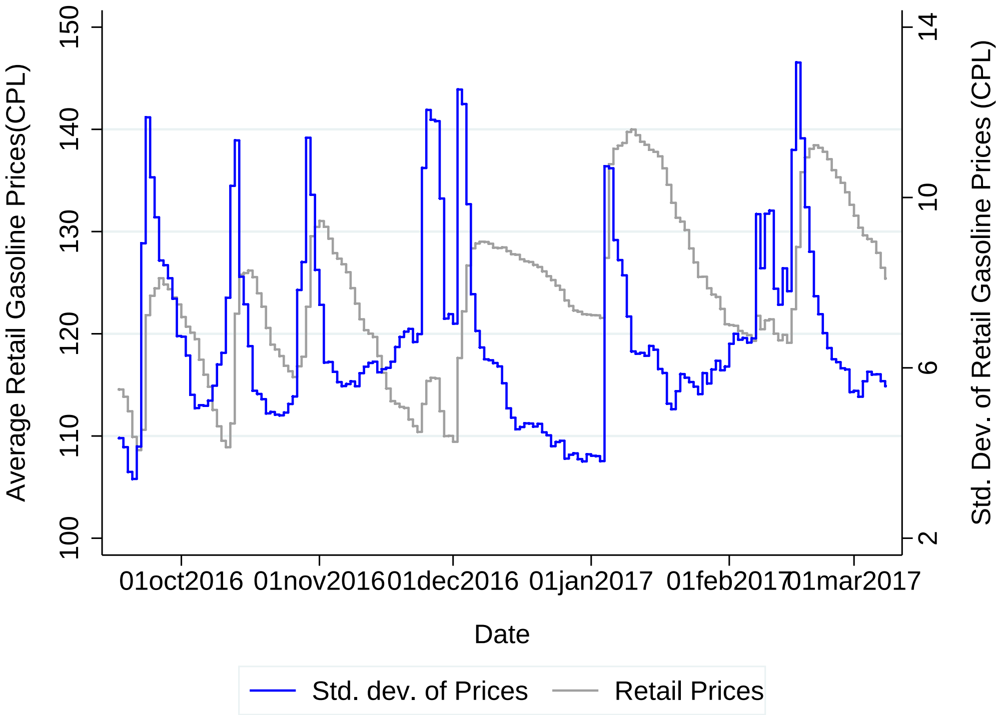


Fig. 4.2 Price Dispersion and Price Levels

stations have engaged in a price jump. For consumers, searching for and purchasing at the lowest-priced station during this period can maximise their savings. Second, the timing of these price jumps is difficult to predict and varies by day-of-the-week[8] and cycle durations. However, should consumers successfully anticipate a price jump, they would benefit from purchasing at a lower price before a jump occurs. Distinguishing the magnitude of responses to both types of search incentives is useful, as they inform policy differently. Therefore, in the second part of my analysis, I model consumers' decisions to search at the cross-section and over time separately.

Likewise, Figure 4.4 that plots station-level prices within a local Statistical Area 2 neighbourhood known as Mascot-Eastlakes also displays similar features to the aggregate cycles illustrated above. In this neighbourhood, it appears that price jumps are also not

[8]For instance, the majority of price jumps in the first cycle occurred on a Friday, whereas they occurred on a Thursday in the second cycle, Saturday in the third cycle, Friday in the fourth cycle, Wednesday in the fifth cycle and Thursday in the sixth cycle.

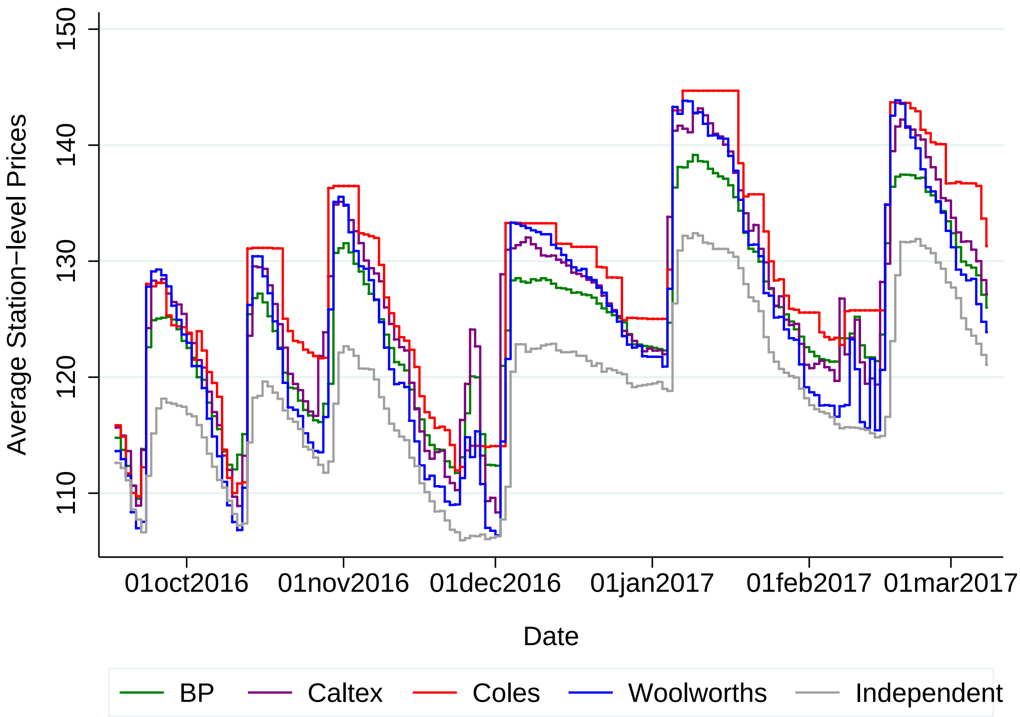


Fig. 4.3 Retail Prices in Greater Sydney, by Brands

perfectly coordinated and that there are approximately 2-3 days before the final station that jumps, jumps.

4.2.2 Price Transparency Initiatives

Searching for the cheapest petrol station can be time-consuming and confusing. This leads to pressing and aggravating concerns that consumers who do not actively search are being exploited by firms (Chandra and Tappata, 2011; Hortaçsu et al., 2017). Worse, consumers who fall in this category are also often the ones who are most financially vulnerable. Some recent case studies that highlight this issue include retail electricity (Thwaites et al., 2017), mortgages (ACCC, 2018b; Allen et al., 2014), pension funds (Hastings et al., 2017) and retail gasoline. As a means to simplify search, more and more government-initiated online price transparency initiatives have been or are being introduced.

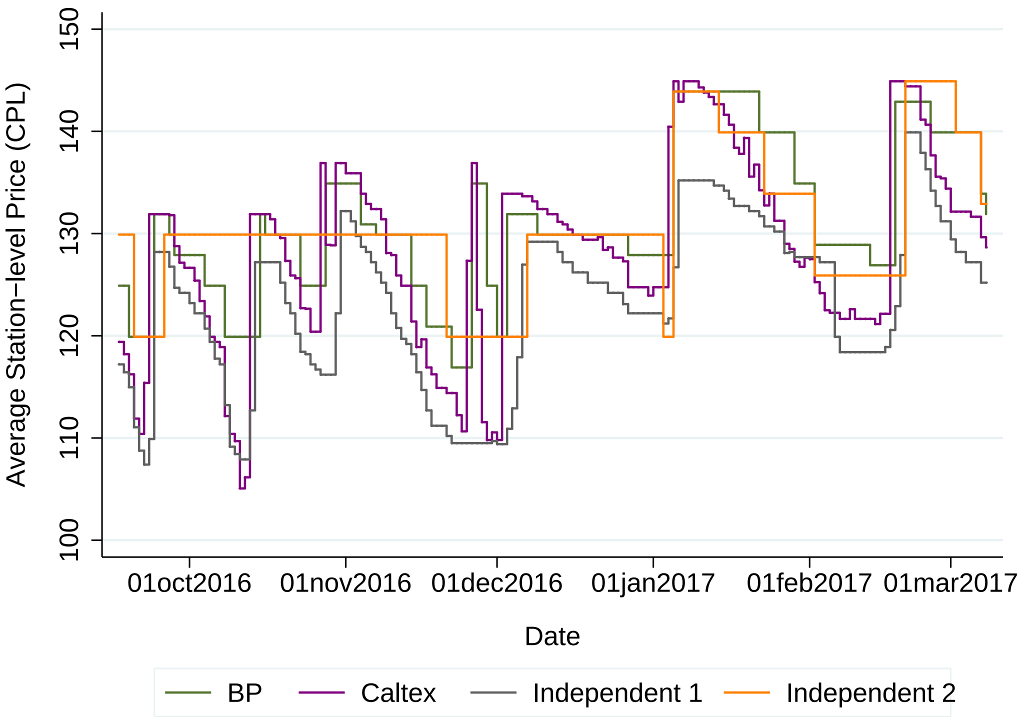


Fig. 4.4 Retail Prices in Mascot-Eastlakes

In Greater Sydney, a price transparency program known as FuelCheck was introduced by the New South Wales' state government on August 1, 2016 with the aim of lowering consumer search frictions. It adds to a growing list of technologies that aim to empower consumers and drive competition in retail petrol markets worldwide, such as GasBuddy, Gas Guru and Waze. Retailers under FuelCheck are required by law to submit every single price update to FuelCheck's website as soon as they change at the pump. Consumers can immediately access these prices by specifying their location on the website[9], which then reveals the latest prices of all nearby gas stations.[10]

It is also important to note that price transparency is not new to Greater Sydney's retail gasoline market. For years, commercial mobile applications such as GasBuddy, PetrolSpy, RACV and Motormouth have been providing price comparison services to consumers. The

[9] In October 2017, New South Wales' state government launched a mobile app version of FuelCheck, which has recorded 424,425 downloads by December 2018.

[10] A screenshot of FuelCheck's platform is depicted in Figure C.1 of the Appendix.

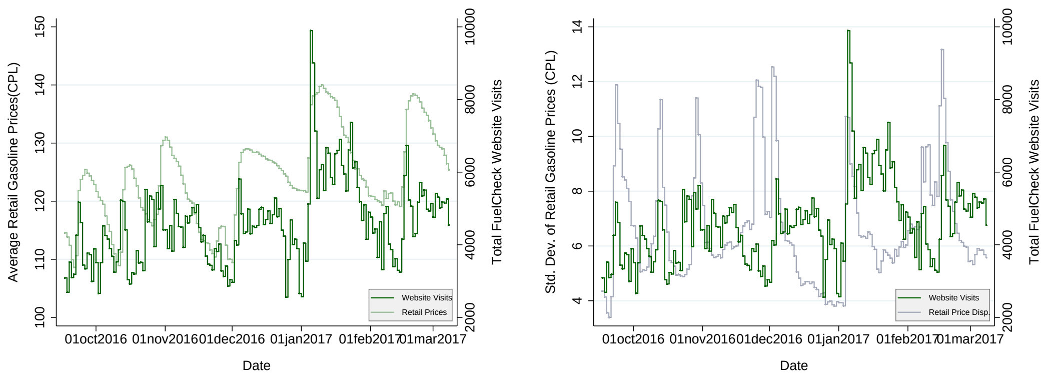


Fig. 4.5 Aggregate Search, Price Levels and Price Dispersion

main difference, however, is that privately-owned applications often rely on crowdsourced prices or voluntary participation from retailers. Therefore, using data from government-enforced systems overcomes potential selection bias issues in measuring price levels and dispersion.

This system that offers a platform for consumers to search for gas prices in real-time is the first of its kind in Australia[11] and similar initiatives are being implemented in the Northern Territory (Myfuel NT) and Queensland.[12] With price cycles present in most urban markets across the country, the ACCC estimates that motorists who anticipate price jumps or search during price jumps can save themselves in the region of $10-15 per tank of petrol for a vehicle with a 60-litre tank, which is a non-negligible amount of savings especially for financially constraint households.

As a preview of the FuelCheck data, Figure 4.5 illustrates the evolution of aggregate search intensities and cyclical price fluctuations. The aggregate patterns suggest that search follows price cycle movements and increases and decreases with price dispersion, and I empirically estimate this relationship below.

[11] Price transparency in retail gasoline is also available in Perth, Australia, but the rules require gas stations to post prices one day before prices actually change at the gas station. There is no price commitment rule in Greater Sydney.

[12] Instead of having a single platform for consumers to access prices (as is the case with FuelCheck and Myfuel NT), the Queensland government gathers price data from all retailers and publish them on existing smartphone apps and websites within 30 minutes of a price change at the pump.

4.3 Data

For my analysis of search and retail pricing, I combine datasets containing information on retailers' prices, website visits and demographics.

4.3.1 Market Definition

Before describing the data, it is useful to discuss the market definition as this informs the unit of my analysis. Retail gasoline is a prototype for localised competition. Due to the fact that consumers face transportation and time costs when switching between gas stations, spatial differentiation is introduced into an otherwise homogeneous product market (Chamberlin, 1949). Although the retail gasoline literature has documented multiple ways of delineating the market, researchers have traditionally assumed that a single station competes with all other stations within a linear distance. For example, Barron et al. (2004) and Lewis (2008) use a 1.5-mile definition, Eckert and West (2005) use a 2-kilometre definition and Luco (2018) uses 1, 3 and 5 kilometre definitions. Other papers such as Hastings (2004), Hastings and Gilbert (2005) and Pennerstorfer et al. (2017) presume that stations located within driving distance from each other compete for the same consumers.

In this essay, I assume that stations belong in the same market if they are located within the same neighbourhood. These local markets are, by definition (since they are different neighbourhoods), mutually exclusive, i.e. each station only belongs to one local market. There are 186 such local markets. Local markets are defined as such because it simplifies the mapping of demographic characteristics to local search. These data are available at various levels of regional disaggregation (census block, neighbourhoods, suburbs, cities, etc.) from the Australian Bureau of Statistics. I have chosen to work at the medium level of regional disaggregation, that is, at the Statistical Area 2 (neighbourhood) level for three reasons. The first reason is technical, that is, I am only able to consider markets that have more than one gas station within its neighbourhood in order to compute price dispersion. After mapping geocoded station locations on various levels of regional disaggregation, I have determined that the Statistical Area 2 (SA2) is the lowest level of disaggregation that best meets this criterion.

The second reason relates to the first, such that having too few stations located in the same market (by construction) limits the scope for search-based incentives. Finally, the demographic data relevant for my study are all available at the Statistical Area 2 level but not at lower levels of disaggregation.

Nevertheless, this method is not without its criticisms. Defining the market as such implies that gas stations only compete within strict SA2 boundaries, which is unlikely given that stations located near the SA2 boundaries also compete for consumers who are located beyond the boundaries. However, the fixed radius approach described earlier faces the issue of overlapping markets and in addition to that, complicates the way in which demographic data are matched. Nevertheless, I provide a robustness analysis of the baseline model using both SA2 and fixed radius approaches in the results section.

4.3.2 FuelCheck Search Data

The price search data used in my analysis are obtained through a collaboration with New South Wales' state government and are derived from an anonymised dataset of online web queries on FuelCheck's website. From the data obtained, I construct a database that contains the timestamp and user-specified location coordinates of each query. In total, the database contains 1.25 million unique website queries for regular unleaded petrol (Unleaded 91) in Greater Sydney that were executed between September 17, 2016 and March 8, 2017.

Consistent with previous assumptions of local demand and search in petrol, I then map each geocoded location into its relevant SA2 neighbourhood/market.[13] Next, I aggregate the

[13]One of the limitations of this essay is that it ignores the possibility that the geography of actual retail markets is more complex than the neighbourhood paradigm used in this essay. In a recent paper, Houde (2012) suggests that gasoline stations located anywhere along the driving route of a consumer require similar search costs. However, as my data only locates a user's location at the point of search and does not identify their

total number of website visits per day executed within an SA2 as my measure of local market search intensity.[14] Overall, the website typically receives between 4,000 and 14,000 visits per day, which includes repeated visits by the same user.[15]

A few back-of-the-envelope calculations give a general sense of consumer engagement on FuelCheck's website. There are approximately 1.2 million working adults in Greater Sydney who drive to work (ABS, 2016). Based on the ACCC (2018a)'s assumption that each motorist fills their car once a week, this implies that Greater Sydney's petrol market has roughly 171,400 customers per day. A daily engagement rate of 4-5% implies that search is still relatively low at this stage, but it also suggests that suppliers are less likely to change their pricing behaviour in response to search intensity on FuelCheck. For this reason and also the fact that stations in Greater Sydney have been coordinating on price cycles for years, I assume that prices are exogeneous to search activities on the FuelCheck platform.

4.3.3 Retail Prices

In addition to price search data, I access individual gas station prices from New South Wales' state government website from August 1, 2016.[16] Specifically, this dataset contains the universe of price uploads along with their timestamps, geocoded location and brands of all 714 operating gas stations in Greater Sydney, Australia.[17][18] The prices are for regular unleaded gasoline (U91) and are reported in terms of Australian cents per litre (cpl).

commuting route, following Houde (2012)'s method would require making assumptions about their commuting route.

[14]Finer aggregations at hourly and minute frequencies are also possible given the data structure. However, the model lacks statistical power when analysed at these frequencies because the website adoption rate is still relatively low.

[15]I assume that each query is executed by a unique individual and this assumption follows the precedents established in existing papers in the literature, such as Byrne and de Roos (2017), and Luco (2018).

[16]These data are available for download on https://data.nsw.gov.au/data/dataset/fuel-check.

[17]See Byrne et al. (2018) for a technical review of the FuelCheck dataset.

[18]Greater Sydney (Greater Capital City Statistical Area), as classified by the Australian Bureau of Statistics, extends from Wyong and Gosford in the north to the Royal National Park in the south and follows the coastline in between. Towards the west, the region includes the Blue Mountains, Wollondilly and Hawkesbury. Greater Sydney covers 12,367.7 square km and is made up of 35 local councils.

Keeping up with convention in the literature, I label vertically-integrated gasoline retailers and stations that belong to supermarket chains “branded” and the rest “independents”. The branded retailers in Greater Sydney are: BP, Caltex, Coles and Woolworths.

For purposes of my analysis, I only keep prices that are posted in the period when search intensity data are also available. That is, I only retain price observations between September 17, 2016 and March 8, 2017. In total, there are 61,351 station-level price reports. Further, I remove markets with fewer than 2 stations since my analysis requires measuring price dispersion. In sum, my working sample contains 55,611 station-level price observations across 186 SA2 markets.

4.3.4 Census Data

Finally, I supplement the dataset with a socio-economic index that combines measures of socio-economic status, such as income, education attainment and employment status. The index, known as the Index of Relative Socio-Economic Disadvantage (IRSD) is created by the Australian Bureau of Statistics and is used to rank areas in Australia on a continuum from most disadvantaged to least disadvantaged.[19] For my analysis, I obtain the corresponding IRSD percentile rank for each SA2 in my sample as a measure of their relative socio-economic disadvantage in the state of New South Wales[20] and then group them into quintiles.

4.3.5 Characterising Price Cycles

Retail prices in my sample exhibit asymmetric cycles, as previously shown in Figure 4.1. As such, I need to define price jumps (restorations) and cycles. Search is typically conducted locally. Yet, it is possible that consumers are cued to search by changes that occur beyond

[19]The index is constructed based on a weighted combination of selected variables derived from the most recent five-year census in 2016. It is part of a broader group of indexes, known collectively as “SEIFA” (Socio-Economic Indexes for Areas). It provides more general measures than is given by measuring, for example, income or unemployment alone.

[20]A technical review of how this index is constructed is available in (ABS, 2011).

their local gas station network. Because there is no unique way of defining cycles, in my estimation I follow two approaches. The first approach is to define cycles broadly at the city (Greater Sydney) level. The downside of this "aggregate" approach is it assumes that consumers respond to price changes in all parts of the city equally. Therefore, I also consider an alternative approach of defining cycles locally at the SA2 neighbourhood level, which matches the unit of observation. Given that I only observe where a price search is being executed, this definition potentially misleads my estimation if consumers also respond to changes in price dispersion beyond their local SA2 gas station network (e.g. on their daily commute to work). For these reasons, I simply report and compare the results from both price cycle definitions in this essay.

Price cycles are defined in a similar approach to Lewis (2012) in the following two ways:

1. **City Cycle definition.** Using a cut-off rule that is common in the literature, a price jump day is a day when at least 20% of gas stations in Greater Sydney increase their prices by $\geq$14cpl from the previous day. This day is coded as "day 0" of a given cycle. Calendar dates $t-1, t-2, t-3$ then count back as days -1, -2 and -3 of the price cycle. Meanwhile, calendar dates $t+1, t+2, t+3$ count up as days 1, 2 and 3 of the price cycle. Dates that fall outside of $\pm$3 days of "day 0" are known as other cycle days that fall outside of a price jump window. Defining cycle days as such allows me to focus on search responses to cross-sectional and intertemporal price dispersion, which is largest around price jump windows.

2. **Local SA2 Cycle definition.** The definition of local SA2 price cycles is similar to aggregate cycles, apart from a few small differences. Based on this approach, a price jump day of a given SA2 neighbourhood is a day when at least 40% of gas stations located in that SA2 market increase their prices by $\geq$14cpl over consecutive or two consecutive days. Under this definition, a single restoration period is either one or two

days.[21] If prices are restored in a single day, then this day is set as “day 0” of a given cycle. Otherwise, if a given restoration period lasts two days, then both days are set as “day 0” of a given cycle. Calendar dates $t-1$, $t-2$, $t-3$ then count back as days -1, -2 and -3 of the price cycle. Meanwhile, calendar dates $t+1$, $t+2$, $t+3$ count up as days 1, 2 and 3 of the price cycle. Dates that fall outside of ± 3 days of “day 0” are known as other cycle days that fall outside of a price jump window.

4.3.6 Descriptive Statistics

Table 4.1 reports the descriptive statistics on the demographic profiles and market characteristics of the SA2 neighbourhoods in my sample. It appears that the SA2 neighbourhoods are similar in all respects except for average median weekly household income, which increases in IRSD quintiles (ranked from most disadvantaged to least disadvantaged). The reported statistics also suggest that there is significant variation in income levels across IRSD quintiles, which is a necessary feature for studying heterogeneous search responses across socio-economic groups.

In terms of retail market structure, SA2’s in the lowest IRSD quintile have the largest average number of stations. This is perhaps unsurprising given that population density is higher in lower-income regions. A larger proportion of stations in SA2’s in the higher IRSD quintiles are branded. If it is true that station brands influence search, then it is possible that search in higher income regions that was originally attributed to changes in price dispersion may be confounded with a larger presence of branded stations. However, the standard deviations are large (approximately 30% of the mean), which indicate that there are large variations in the percentage of branded stations within IRSD quintiles. This reduces concerns that the unequal presence of branded stations will disproportionately affect some IRSD

[21] The cut-off rule here is higher than the city cycle’s rule because there are far fewer gas stations within each SA2 neighbourhood compared to a city. For example, if the previous definition were applied to an SA2 neighbourhood with only 5 stations, then even if there is only one station that has engaged in a price jump on a certain day, this day still counts as a price jump day.

Table 4.1 Descriptive Statistics of SA2 Markets

		Demographics			Stations	
IRSD Quintile	No. of markets	Population density (pp./km^2)	Median HH income (weekly)	% Working age pop.	No. of stations	% Branded stations
1	35	3334.49	1187.14	66.17	3.97	0.36
	0	(1772.42)	(140.92)	(2.85)	(1.72)	(0.26)
2	20	1884.55	1385.82	65.55	3.51	0.45
	0	(1218.41)	(202.94)	(4.33)	(1.69)	(0.24)
3	37	3335.37	1587.30	68.60	3.32	0.50
	0	(2595.71	(181.92	(6.10	(1.27	(0.31
4	48	2683.03	1805.73	67.48	3.40	0.51
	0	(1902.71)	(189.72)	(4.69)	(1.24)	(0.27)
5	45	2713.16	2254.29	66.41	3.09	0.43
	0	(2403.20)	(283.30)	(5.73)	(1.46)	(0.26)

Notes: There are 186 SA2 administrative units in the sample. Sample averages and standard deviations (in parentheses) by SA2 administrative units are reported.

quintiles more than others. Furthermore, I also use fixed effects in the regressions to control for SA2 heterogeneities. Moreover, Figure 4.6 below that plots the frequency of price jumps in each socio-economic quintile shows that the timing of when stations change their prices around price jump periods does not vary by socio-economic status. This indicates that there is balance across areas with regards to cycle timing.

Table 4.2 presents descriptive statistics of price cycles and search behaviour at the aggregate and local levels. Panel A reports city-wide averages and standard deviations of daily changes in price levels, standard deviation of prices and total FuelCheck website visits in Greater Sydney on a given cycle day, while Panel B reports the same statistics aggregated at local SA2 neighbourhoods. On aggregate, the descriptive statistics portraying the build-up of website visits as “day 0” approaches and their decline thereafter reaffirm the patterns observed in Figure 4.5. It implies that motorists are searching more in anticipation of a price jump event. Both panels record the highest increase in average prices on “day 0” of both Greater Sydney and local SA2 cycles. The respective increases in average price levels of 7.71

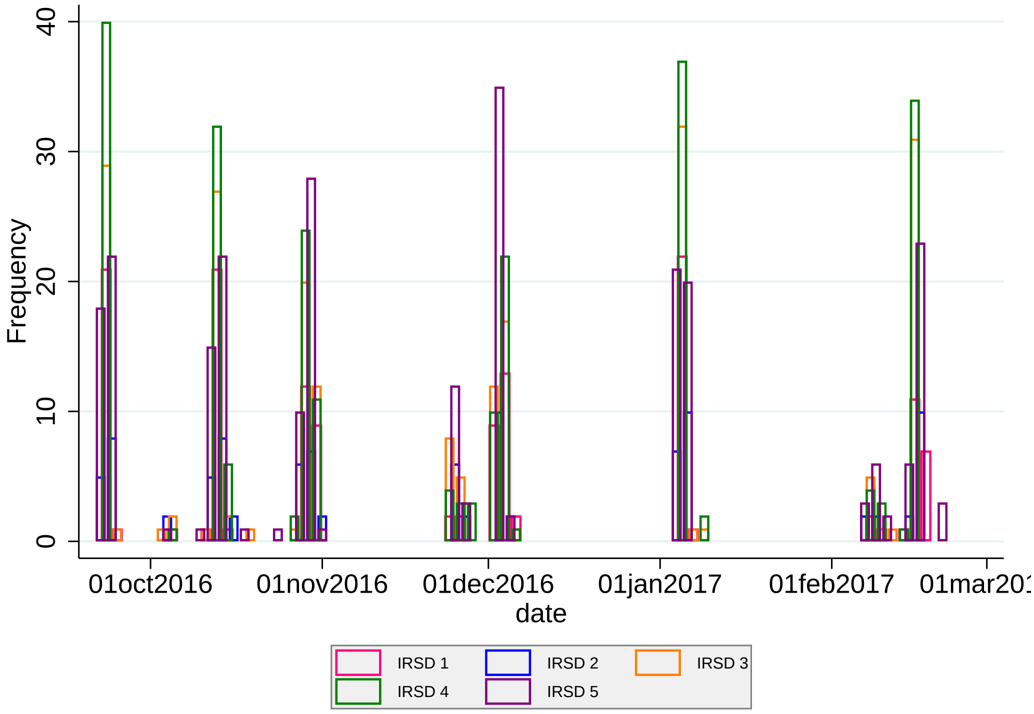


Fig. 4.6 Frequency of Price Jumps, by Socio-economic Quintiles

cpl and 12.65 cpl represent 6% and 11% increases from the mean retail price of 124.7 cpl. These imply that consumers can pay substantially more if they mistime their purchases. At the same time, consumers can also benefit from searching at the cross-section for low-priced stations that have not yet engaged in a price jump, especially on “day 0” of the price cycle, when price dispersion is highest.

Although the descriptive statistics present provide useful insights on aggregate search responses to changes in price dispersion, the same cannot be said at the local level. The variances in search intensities at the local level are demonstrably large, which makes it difficult to establish any pattern for search intensities across cycle days. These large dispersions in search intensities could also stem from heterogeneous search reactions from different income and socio-economic groups, which I explore empirically in the next section.

Table 4.2 Descriptive Statistics of Price Cycles

Panel A. City Cycles

Cycle Day	Price Changes		Price Dispersion		FuelCheck Visits	
-3	-1.07	(1.15)	5.86	(1.74)	3749.59	(923.24)
-2	-0.28	(0.69)	6.55	(1.68)	3954.62	(470.29)
-1	1.17	(1.29)	8.25	(2.30)	4636.39	(868.68)
0	7.71	(2.44)	11.79	(0.83)	5425.47	(521.68)
1	5.61	(2.42)	10.52	(1.25)	6306.46	(2074.16)
2	1.56	(1.40)	9.04	(0.90)	5624.32	(2113.50)
3	0.78	(0.48)	7.97	(0.85)	5090.34	(1501.49)
Other Cycle Day	-0.63	(0.78)	6.21	(1.51)	5018.88	(1081.24)

Panel B. Local SA2 Cycles

Cycle Day	Price Changes		Price Dispersion		FuelCheck Visits	
-3	-0.81	(1.86)	4.23	(3.73)	21.32	(19.40)
-2	-0.38	(2.12)	4.55	(3.93)	22.14	(20.24)
-1	0.42	(3.00)	5.63	(4.83)	25.75	(25.25)
0	12.65	(5.93)	11.47	(5.36)	32.47	(32.41)
1	3.03	(5.65)	8.52	(5.94)	32.58	(32.33)
2	0.92	(3.81)	6.92	(5.33)	29.26	(29.21)
3	0.17	(2.68)	6.01	(4.80)	25.78	(24.25)
Other Cycle Day	-0.53	(1.97)	5.04	(3.73)	27.10	(24.72)

Notes: The sample used to generate the descriptive statistics consists of 31,978 observations. Sample averages and standard deviations (in parentheses) by SA2 neighbourhoods are reported. An "other cycle day" represents any day beyond a ± 3 day window of "day 0" of a price cycle.

4.4 Baseline Model

My analysis consists of two parts. The first part examines the overall effects of price jump events on search intensities on FuelCheck across different socio-economic groups. The second part builds on the first and explores differential cross-sectional and intertemporal search. The following equation is adopted from Byrne and de Roos (2017) and forms the baseline regression model:

$$\begin{aligned}\ln(Search)_{mt} = \beta_0 + \sum_{l=-3}^{3} \beta_{1l}\mathbf{1}\{CycleDay_{st} == l\} + \sum_{k=1}^{4} \beta_{2k}\mathbf{1}\{DISADV_m == k\} \\ + \sum_{l=-3}^{3}\sum_{k=1}^{4} \gamma_{kl}\left(\mathbf{1}\{CycleDay_{st} == l\} \times \mathbf{1}\{DISADV_m == k\}\right) \\ + \kappa_m + \kappa_{weekend} + \kappa_{month} + \varepsilon_{mt} \qquad (4.1)\end{aligned}$$

where the dependent variable is the natural logarithm of total FuelCheck website visits in a given SA2 market m on date t. Recall that price cycles are defined at two levels – *aggregate* and *local* – and the definition used in the regression estimation is distinguished by the subscript s. In the specification that refers to aggregate price cycles, s represents Greater Sydney cycles. Instead, s corresponds to the same SA2 neighbourhood as the unit of observation (i.e. $s = m$) in the alternative definition. The indicator function $\mathbf{1}\{CycleDay_{st} == l\}$ equals 1 if date t coincides with day l of the price cycle under specification s. As per definition, $l = 0$ is the price jump day, $l = \{-3, -2, -1\}$ represent the single days leading up to the price jump and $l = \{1, 2, 3\}$ represent the days after the price jump. Relative socio-economic disadvantage rankings are represented by the indicator function $\mathbf{1}\{DISADV_m == k\}$. The function equals 1 if market m is ranked in quintile k based on its Index of Relative Socio-Economic Disadvantage (IRSD), with $k = 1$ including 20 percent of the most disadvantaged SA2 neighbourhoods in the state and $k = 5$ including the least disadvantaged SA2 neighbourhoods. Other controls include SA2 fixed effects κ_m, as well as weekend $\kappa_{weekend}$ and month-of-year κ_{month} dummies to control for secular trends in search intensity on the platform. To account for correlation in search shocks within each local market, standard errors, ε_{mt}, are clustered by SA2 neighbourhoods.

In this model, cycle day dummies effectively represent the collective changes in cross-sectional and intertemporal search incentives during the price jump period. Therefore, the interactions between cycle days and socio-economic rankings uncover heterogeneities in search responses to price jump events through the γ_{kl} coefficients.

The model I present identifies search sensitivity to daily changes in price levels and dispersion among users on FuelCheck's platform. Therefore, the identifying assumption holds only if firms do not also set prices in response to consumer search. At this stage, this is of lesser concern because the FuelCheck website is still in its infancy and the percentage of motorists using the platform is less than 5%. In addition, price cycles have existed in the Greater Sydney region for years ACCC (2018a) even before FuelCheck was introduced. In sum, I proceed under the assumption that the cycles, and hence prices, are exogeneous to online search behaviour on FuelCheck.

4.4.1 Results and Discussion

Table 4.3 presents the results of the baseline regression. Column (1) provides estimates from the aggregate model, which models FuelCheck's adopters' search response to Greater Sydney cycles.[22] Column (2) reports estimates from the disaggregated model, which models platform users' search response to SA2 neighbourhood cycles. Specifically, it reports the $\beta_1 + \gamma$ coefficient estimates from regression 4.1. Each β_1 coefficient estimate represents the percentage increase or decrease in search intensity of FuelCheck users in the base category (i.e. the least disadvantaged IRSD quintile) on a given day of the cycle relative to a day outside of a price jump event. For example, a $\hat{\beta}_{1,0}$ of 0.154 in column (1) implies that users in the least disadvantaged socio-economic quintile engage in 15.4% more searches on "day 0" of a Greater Sydney price cycle than on a day outside of a price jump event. Looking across cycle days, search intensity in the base category under both cycle definitions

[22]In a robustness analysis, I compare pooled search responses to city cycle days for different market definitions and report the results in Table C.1 of the Appendix. The fixed radius approach considers all stations within a fixed radius from a reference station to belong to the same market. The results show that both market definitions yield similar results, such that search intensity rises leading up to a price jump event and decreases thereafter. Similar patterns are found for 1km, 3km and 5km radii.

appears to heighten on day 0. Search on the platform intensifies further on the day after and subsequently returns to the original level by day 2. However, a comparison of the coefficient magnitudes in columns (1) and (2) shows that search reaction to local SA2 price cycles is stronger despite sharing similar search patterns, which implies that motorists pay more attention to changes in price dispersion within their local neighbourhood than throughout the city.

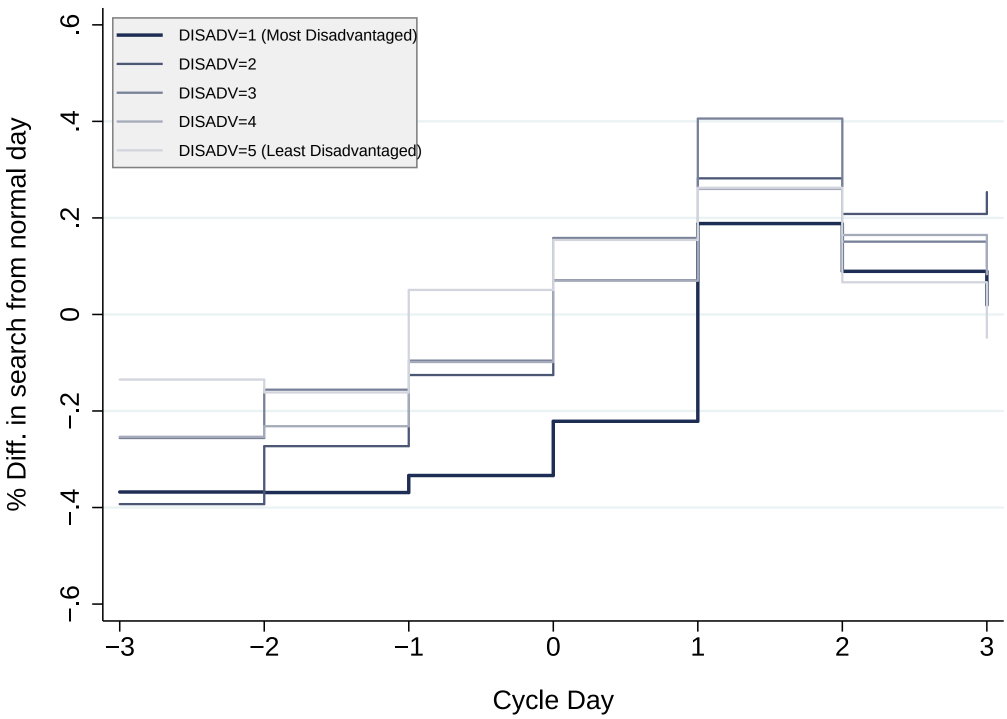
The results reported in Table 4.3 also reveals how online search on FuelCheck varies across socio-economic backgrounds. The $\hat{\gamma}$'s reported for each socio-economic quintile that a platform user belongs to portray these heterogeneities, such that differences between these estimates imply differences in search behaviour across socio-economic groups. For ease of presentation, I reproduce the results from Table 4.3 for both Greater Sydney and SA2 market definitions in Figures 4.7 and 4.8. Specifically, each observation represents the percentage increase or decrease in search intensity on a given day during a price jump event relative to a day outside of a price jump event within a socio-economic quintile. For example, Figure 4.7 shows that on day 0 of a city cycle, search intensity among platform users in the most disadvantaged socio-economic quintile is on average 20.8% lower ($\hat{\beta}_{1,0} + \hat{\gamma}_{1,0}$) than on a day outside of a price jump event and is 24.6% higher ($\hat{\beta}_{1,1} + \hat{\gamma}_{1,1}$) on day 1 than on a day outside of a price jump event. Search trends upwards in the days leading up to a price jump day and remains high during the first two days of the jump before returning back to original levels.

The figures show striking differences in search responses between FuelCheck users in the most disadvantaged socio-economic quintile and the rest. First, users in the most disadvantaged socio-economic quintile do not increase search during a price restoration period by as much as users in other socio-economic groups. Second, users in the most disadvantaged socio-economic quintile also appear to react slower to price jumps compared to users from higher socio-economic quintiles. For instance, Figure 4.8 shows that on day 0 of a cycle, search intensity among the users from the bottom socio-economic quintile is

Table 4.3 Baseline Results

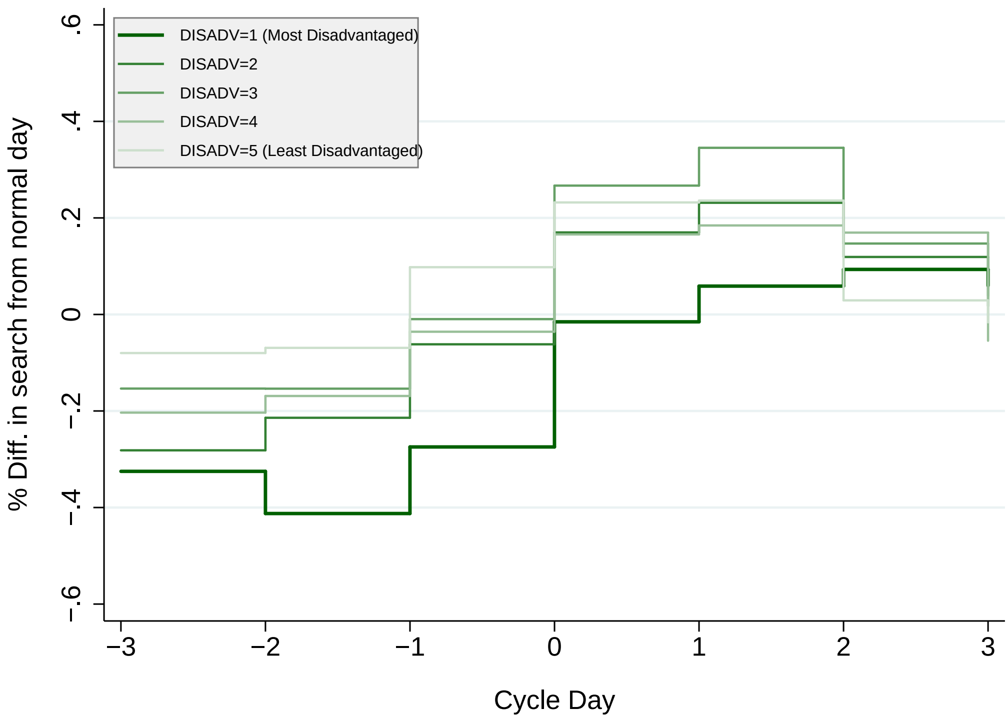
	City Cycles (1)		Local SA2 Cycles (2)	
Base Category, $\hat{\beta}_1$ (Least Disadvantaged)				
Day -3	-0.135***	(0.04)	-0.080*	(0.04)
Day -2	-0.162***	(0.04)	-0.069*	(0.04)
Day -1	0.051	(0.04)	0.098**	(0.04)
Day 0	0.154***	(0.04)	0.232***	(0.04)
Day 1	0.262***	(0.04)	0.236***	(0.04)
Day 2	0.067	(0.04)	0.029	(0.04)
Day 3	-0.048	(0.04)	-0.016	(0.04)
DISADV $= 4, \hat{\gamma}_4$				
Day -3	-0.119**	(0.06)	-0.123**	(0.06)
Day -2	-0.070	(0.05)	-0.100	(0.06)
Day -1	-0.149**	(0.06)	-0.134**	(0.06)
Day 0	-0.085	(0.06)	-0.066	(0.06)
Day 1	-0.002	(0.05)	-0.052	(0.06)
Day 2	0.098*	(0.06)	0.140**	(0.06)
Day 3	0.131**	(0.06)	-0.038	(0.06)
DISADV $= 3, \hat{\gamma}_3$				
Day -3	-0.121*	(0.06)	-0.074	(0.06)
Day -2	0.006	(0.06)	-0.084	(0.06)
Day -1	-0.146**	(0.07)	-0.108*	(0.06)
Day 0	0.004	(0.06)	0.035	(0.06)
Day 1	0.143***	(0.05)	0.109*	(0.06)
Day 2	0.084	(0.06)	0.118**	(0.06)
Day 3	0.148**	(0.06)	0.055	(0.06)
DISADV $= 2, \hat{\gamma}_2$				
Day -3	-0.258***	(0.08)	-0.202**	(0.09)
Day -2	-0.111	(0.07)	-0.145*	(0.08)
Day -1	-0.176**	(0.08)	-0.160*	(0.09)
Day 0	-0.084	(0.08)	-0.062	(0.08)
Day 1	0.020	(0.07)	-0.005	(0.08)
Day 2	0.142*	(0.08)	0.090	(0.09)
Day 3	0.302***	(0.07)	0.034	(0.09)
DISADV $= 1, \hat{\gamma}_1$ (Most Disadvantaged)				
Day -3	-0.233***	(0.07)	-0.245***	(0.07)
Day -2	-0.207***	(0.06)	-0.343***	(0.07)
Day -1	-0.384***	(0.06)	-0.372***	(0.07)
Day 0	-0.376***	(0.06)	-0.247***	(0.07)
Day 1	-0.074	(0.06)	-0.177***	(0.07)
Day 2	0.023	(0.06)	0.064	(0.07)
Day 3	0.067	(0.06)	0.076	(0.07)
N	31663		28212	
Adj. R^2	0.579		0.574	

Notes: The dependent variable, $ln(Search)_t$, is the natural logarithm of the number of FuelCheck website visits on date t. Clustered standard errors at the SA2 level are reported in parentheses. ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels. All specifications control for *DISADV* dummies, SA2, weekend and month-of-year fixed effects.



Notes: This figure plots the $\beta_1 + \gamma_k$ estimates from from column (1) of Table 4.3. They each represent the percentage change in average website visits on a given day during a price jump event relative to a day outside of a price jump event within a socio-economic group (i.e. IRSD quintile). Each line represents IRSD 1 (most disadvantaged), 2, 3, 4 and 5 (least disadvantaged) in increasing opacity.

Fig. 4.7 Baseline Search Response to Greater Sydney Cycles



Notes: This figure plots the $\beta_1 + \gamma_k$ estimates from column (2) of Table 4.3. They each represent the percentage change in average website visits on a given day during a price jump event relative to a day outside of a price jump event within a socio-economic group (i.e. IRSD quintile). Each line represents IRSD 1 (most disadvantaged), 2, 3, 4 and 5 (least disadvantaged) in increasing opacity.

Fig. 4.8 Baseline Search Response to Local SA2 Cycles

18 to 25 percentage points lower than users from the upper quintiles. Furthermore, these differences between the bottom 20 percent and the upper socio-economic quintiles are persistent throughout the price jump event. Put together, these results suggest that users from less disadvantaged socio-economic quintiles are less attentive to price jumps and are not as effective at anticipating price jumps in the market. This is concerning because it appears that they only start searching when the majority of stations have already increased their prices, which leaves them with fewer low-priced stations to purchase from.[23]

Visually, search responses to price jumps are heterogeneous among FuelCheck users from different socio-economic backgrounds. It remains to be tested whether the heterogeneities observed are statistically significant. I formally conduct pairwise comparisons of the γ coefficient estimates between socio-economic groups by cycle day and report the F-statistics in Table 4.4. Panel A presents the test results of Greater Sydney cycles, which correspond to the coefficient estimates in column (1) of Table 4.3, while panel B presents the test results of local SA2 cycles, which corresponds to coefficient estimates in column (2) of Table 4.3. The results reaffirm the graphical findings that the differences in search reaction are mostly significant between users in the bottom 20 percent of the socio-economic distribution and the top 80 percent immediately before and after price jumps. However, the differences are mostly insignificant among users from other socio-economic quintiles. In sum, it appears that search responses are only statistically distinguishable between FuelCheck users in the lowest 20 percent of the socio-economic distribution and the rest.

It is perhaps surprising that those who get more value from search relative to income are less engaged in search. In this way, my findings are revealing the importance of other

[23]It is possible that the results are driven by differences in the timing of price jumps and the size of gains across markets. However, since Figure 4.6 shows little variation in the timing of price jumps across neighbourhoods, I rule out the possibility that the delays observed in search responses are due to stations in the lower socio-economic neighbourhoods engaging in price jumps later. Furthermore, the delays are also present in the analysis featuring city cycles, as shown Figure **??**. It is also possible that the reaction delays are driven by the differences in magnitude of price jumps, where the pass-through of price leadership could be lower in lower-income neighbourhoods (Stolper, 2016), but I find that this is not the case in my sample.

Table 4.4 Pairwise Significance Tests

Panel A. City Cycles

	Day -3	Day -2	Day -1	Day 0	Day 1	Day 2	Day 3
DISADV 1 vs 2	0.09	1.88	7.34***	12.90***	1.52	2.11	9.52***
DISADV 1 vs 3	2.78*	13.28***	12.91***	32.75***	13.46***	0.97	1.60
DISADV 1 vs 4	3.47*	6.26**	16.18***	23.34***	1.59	1.51	1.13
DISADV 1 vs 5	12.76***	12.38***	39.48***	36.79***	1.54	0.13	1.18
DISADV 2 vs 3	2.62	2.86*	0.13	1.13	3.02*	0.55	4.17**
DISADV 2 vs 4	3.03*	0.39	0.13	0.00	0.10	0.33	5.50**
DISADV 2 vs 5	9.65***	2.56	5.20**	1.12	0.07	3.27*	16.59***
DISADV 3 vs 4	0.00	1.98	0.00	2.04	8.42***	0.06	0.08
DISADV 3 vs 5	3.65*	0.01	4.81**	0.00	7.23***	2.11	5.81**
DISADV 4 vs 5	4.28**	1.66	6.40**	2.18	0.00	3.00*	5.15**

Panel B. Local SA2 Cycles

	Day -3	Day -2	Day -1	Day 0	Day 1	Day 2	Day 3
DISADV 1 vs 2	0.23	5.66**	5.23**	4.23**	3.74*	0.07	0.21
DISADV 1 vs 3	6.36**	14.24***	14.54***	14.81***	16.94***	0.57	0.10
DISADV 1 vs 4	3.46*	12.22***	13.09***	6.68***	3.35*	1.24	2.68
DISADV 1 vs 5	13.48***	26.92***	31.73***	12.31***	6.65***	0.86	1.26
DISADV 2 vs 3	2.16	0.58	0.33	0.21	2.02	0.10	0.06
DISADV 2 vs 4	0.85	0.32	0.09	1.39	0.35	0.33	0.64
DISADV 2 vs 5	5.49**	3.54*	3.29*	0.61	0.00	1.05	0.15
DISADV 3 vs 4	0.72	0.06	0.17	2.86*	8.18***	0.15	2.22
DISADV 3 vs 5	1.50	1.94	2.93*	0.34	3.77*	3.87**	0.84
DISADV 4 vs 5	4.68**	2.6	5.14**	1.39	0.87	6.09**	0.36

Notes: The table reports the F-statistics from Wald tests of the pairwise γ coefficient estimates across different DISADV quintiles being equal to each other. ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels.

constraints disadvantaged groups face, such as lower cognitive skills and rational inattention that limit their ability to search.[24]

Comparing prices over time in an unpredictable environment, such as the retail gasoline market of Sydney, is a complex process. The study by de Roos and Smirnov (2017) find that price cycles are an obfuscation strategy used to cloud consumers' price awareness and increase search costs.[25] In a dynamic setting, the complexity of the price path determines how well consumers compare prices of a given product over time. If consumers observe the same price over time, any deviation from the usual price will be easily detected. On the other hand, price changes are less obvious if consumers observe a wide range of prices over time, especially if they are not actively engaged in search most of the time. Evidence by Noel (2018) further reinforces that price cycles are confusing for consumers. He finds that calendar synchronised price cycles[26] help consumers time their purchases better than unsynchronised cycles.

My findings that users from the most disadvantaged socio-economic quintile engage in lower and delayed search appeal to the possibility that users in this group have lower information-processing capabilities (Sims, 1998, 2003).[27] Banerjee and Mullainathan (2008) explain that households from lower socio-economic backgrounds have more competing pressures and issues at home, such as worrying about affording necessities and having a stable connection to the power grid among others. Since attention is scarce, being burdened by more distractions reduces the amount of attention they can dedicate to a particular task, which leads to making less-informed choices. On the other hand, higher-income households

[24]It is possible that technology adoption is lower in more disadvantaged neighbourhoods, leading to lower search activities in these areas. However, this essay only considers individuals who already engage with the platform, which rules out lower technology adoption as an explanatory factor for the differences I find.

[25]Other evidence on firms using obfuscation strategies to increase search costs include Ellison and Ellison (2009), Ellison and Wolitzky (2012), Piccione and Spiegler (2012), and Wilson (2010).

[26]Retail gasoline price cycles in Perth, for example, are calender synchronised.

[27]A new behavioural literature is now dedicated to understand why people behave in a way that departs from standard economic predictions. Examples include Evans and Honkapohja (2012), Abaluck and Gruber (2011) and Gul and Pesendorfer (2004). Suboptimal behaviours could be due to individuals possessing imperfect memory (Bénabou and Tirole, 2002; Mullainathan, 2002) and individuals relying on heuristics to reduce cognitive burdens (Chetty et al., 2009; Gabaix and Laibson, 2006, 2000; Gallagher and Muehlegger, 2011).

can afford goods and services that reduce distraction from problems at home, such as a babysitter or connection to the power grid, hence enabling them to dedicate more attention to comparing prices. Relatedly, Hastings et al. (2017) find that lower-income households are more easily influenced by sales agents, leading to less elastic preferences.

An alternative explanation relates to the cognitive development of individuals. A body of evidence has recently emerged from the neuroscience literature showing that growing up in poorer environments have more damaging effects on one's attention capacities (Bernier et al., 2015; Boelema et al., 2014; Mezzacappa et al., 2011).[28] Noble et al. (2015) find a positive relationship between income and brain surface area,[29], in which experiencing deprivation at a young age can hamper brain development and impair attention and memory.[30]

4.5 Disentangling Intertemporal and Cross-sectional Search

While the results from the baseline model provide insightful evidence of heterogeneous search responses, they mask underlying mechanisms that drive search behaviour. For instance, do consumers respond more to changes in price levels or are they more responsive to cross-sectional price dispersion? Do users from different socio-economic backgrounds respond differentially to different search incentives? To answer these questions, I build on the baseline model and attempt to unpack intertemporal and cross-sectional search incentives using the following regression:

[28]Researchers find that individuals from poorer backgrounds are less able to engage in selective attention. Selective attention is the process of directing awareness to relevant information, while ignoring other irrelevant details, such as the ability to isolate information about fuel prices from prices of other products.

[29]Surface area undergoes maturational changes from childhood to adulthood and are influenced by both genetic programming and experience.

[30]See Sheehy-Skeffington and Rea (2017) and Dean et al. (2017) for summaries of the most recent evidence on the relationship between socio-economic status and the psychological and social processes that underpin decision-making.

$$
\begin{aligned}
\ln(Search)_{mt} = \beta_0 + \sum_{l=-3}^{3} \beta_{1l} \mathbf{1}\{CycleDay_{st} == l\} + \sum_{k=1}^{4} \beta_{2k} \mathbf{1}\{DISADV_m == k\} \\
+ \sum_{l=-3}^{3} \sum_{k=1}^{4} \gamma_{kl} \left(\mathbf{1}\{CycleDay_{st} == l\} \times \mathbf{1}\{DISADV_m == k\} \right) \\
+ \phi_1 \sigma_{p,st} + \sum_{k=1}^{4} \phi_{2k} \left(\sigma_{p,st} \times \mathbf{1}\{DISADV_m == k\} \right) \\
+ \sum_{j=t-1}^{j=t+1} \delta_{1j} \triangle p_{st} + \sum_{j=t-1}^{j=t+1} \sum_{k=1}^{4} \delta_{2jk} \left(\triangle p_{st} \times \mathbf{1}\{DISADV_m == k\} \right) \\
+ \kappa_m + \kappa_{weekend} + \kappa_{month} + \varepsilon_{mt} \qquad (4.2)
\end{aligned}
$$

where the variables remain unchanged from equation 4.1, except for two added features. The first is $\sigma_{p,st}$, the standard deviation of retail prices on date t in either Greater Sydney or the local SA2 market, depending on which price cycle definition is used. It measures price dispersion on a given day and market.[31] The second feature is $\triangle p_{s,t} = p_{s,t} - p_{s,t-1}$, which represents the change in daily average price level in the market from the previous day's average. Modelling them as additively separable variables enables me to measure the extent of which search behaviour is governed by cross-sectional and intertemporal search incentives.[32]

The key coefficients are ϕ_2's and δ_2's. Coefficient ϕ_2 represents the marginal search response to changes in cross-sectional price dispersion. Coefficients $\delta_{2,t-1}$, $\delta_{2,t}$ and $\delta_{2,t+1}$ represent the marginal search response to lagging, contemporaneous and leading changes in price levels.

[31] Alternative measures of price dispersion such as sample range and gains from search, $E[p - p_{min}]$ are also used. All three measures report similar results and hence, only results from using standard deviation of prices are reported in the text.

[32] Throughout the analysis, cycle day fixed effects are kept in all regression specifications to be consistent with Byrne and de Roos (2017). This is important for isolating reaction to price variation and habit, as it would be imprecise to assume that all users react to price variation alone. Therefore, including cycle day fixed effects is a tougher test than excluding cycle day fixed effects because it also controls for residual price variation.

4.5.1 Results and Discussion

The empirical results from the extended model are presented and discussed in this section. Table 4.5 provides estimates from the aggregate model, while Table 4.6 reports estimates from the disaggregated model evaluated at the SA2 neighbourhood level. In this section, the discussion on the aggregate model will be followed by a discussion on the disaggregated (local SA2) model.

In both tables, column (1) provides the baseline regression coefficients from reg 2.1 that provide context for the findings in columns (2) and (3). As discussed before, the estimates in column (1) represent the search reaction to cyclical variation in price levels and dispersion. Adding contemporaneous price dispersion and changes in price levels to the regression in columns (2) and (3) reduces the effects of the cyclical variation in prices. For clarity, I visually present the results from Tables 4.5 and 4.6 in Figures 4.9 and 4.10. I reproduce Figures 4.7 and 4.8 on the left panels. On the right panel, I present the coefficient estimates of the baseline regression after controlling for cross-sectional and intertemporal price dispersion.

Table 4.5 Disentangling cross-sectional and intertemporal search – City Cycles

	(1)		(2)		(3)	
Base Category						
Day -3	-0.135***	(0.04)	-0.128***	(0.04)	-0.143***	(0.04)
Day -2	-0.162***	(0.04)	-0.169***	(0.04)	-0.206***	(0.04)
Day -1	0.051	(0.04)	0.011	(0.04)	-0.081	(0.08)
Day 0	0.154***	(0.04)	0.042	(0.05)	-0.154*	(0.08)
Day 1	0.262***	(0.04)	0.175***	(0.04)	0.094	(0.08)
Day 2	0.067	(0.04)	0.010	(0.04)	0.008	(0.06)
Day 3	-0.048	(0.04)	-0.084*	(0.04)	-0.100**	(0.05)
σ_{pt}			0.020***	(0.01)	0.009	(0.01)
$\triangle p_t$					0.026***	(0.01)
$\triangle p_{t+1}$					0.008	(0.01)
$\triangle p_{t-1}$					-0.007	(0.01)
DISADV = 4						
Day -3	-0.119**	(0.06)	-0.127**	(0.06)	-0.123**	(0.06)
Day -2	-0.070	(0.05)	-0.061	(0.05)	-0.064	(0.06)
Day -1	-0.149**	(0.06)	-0.098	(0.06)	-0.131	(0.10)
Day 0	-0.085	(0.06)	0.057	(0.07)	-0.001	(0.11)
Day 1	-0.002	(0.05)	0.107*	(0.06)	-0.011	(0.11)
Day 2	0.098*	(0.06)	0.170***	(0.06)	0.093	(0.09)
Day 3	0.131**	(0.06)	0.176***	(0.06)	0.150**	(0.06)
σ_{pt}			-0.005	(0.00)	-0.023***	(0.01)
$\triangle p_t$					0.032***	(0.01)
$\triangle p_{t+1}$					0.012*	(0.01)
$\triangle p_{t-1}$					0.005	(0.01)
DISADV = 3						
Day -3	-0.121*	(0.06)	-0.129**	(0.06)	-0.115*	(0.06)
Day -2	0.006	(0.06)	0.014	(0.06)	0.035	(0.06)
Day -1	-0.146**	(0.07)	-0.101	(0.07)	-0.036	(0.12)
Day 0	0.004	(0.06)	0.129*	(0.07)	0.101	(0.12)
Day 1	0.143***	(0.05)	0.240***	(0.06)	0.098	(0.11)
Day 2	0.084	(0.06)	0.147**	(0.06)	0.063	(0.09)
Day 3	0.148**	(0.06)	0.187***	(0.06)	0.154**	(0.07)
σ_{pt}			-0.002	(0.01)	-0.018***	(0.01)
$\triangle p_t$					0.037***	(0.01)
$\triangle p_{t+1}$					-0.002	(0.01)
$\triangle p_{t-1}$					0.007	(0.01)
DISADV == 2						
Day -3	-0.258***	(0.08)	-0.270***	(0.08)	-0.245***	(0.09)

Continued on next page

Table 4.5 – continued from previous page

	(1)		(2)		(3)	
Day -2	-0.111	(0.07)	-0.099	(0.07)	-0.071	(0.07)
Day -1	-0.176**	(0.08)	-0.108	(0.08)	-0.032	(0.13)
Day 0	-0.084	(0.08)	0.106	(0.09)	0.017	(0.15)
Day 1	0.020	(0.07)	0.166**	(0.08)	-0.063	(0.15)
Day 2	0.142*	(0.08)	0.237***	(0.08)	0.116	(0.12)
Day 3	0.302***	(0.07)	0.362***	(0.08)	0.311***	(0.08)
σ_{pt}			-0.013*	(0.01)	-0.031***	(0.01)
$\triangle p_{t+1}$					-0.004	(0.01)
$\triangle p_{t-1}$					0.012	(0.01)
DISADV = 1						
Day -3	-0.233***	(0.07)	-0.254***	(0.07)	-0.227***	(0.07)
Day -2	-0.207***	(0.06)	-0.187***	(0.06)	-0.174***	(0.06)
Day -1	-0.384***	(0.06)	-0.265***	(0.06)	-0.300***	(0.11)
Day 0	-0.376***	(0.06)	-0.048	(0.08)	-0.225*	(0.12)
Day 1	-0.074	(0.06)	0.179***	(0.07)	-0.195	(0.12)
Day 2	0.023	(0.06)	0.190***	(0.07)	-0.045	(0.09)
Day 3	0.067	(0.06)	0.171***	(0.06)	0.087	(0.07)
σ_{pt}			-0.038***	(0.01)	-0.061***	(0.01)
$\triangle p_t$					0.046***	(0.01)
$\triangle p_{t+1}$					0.009	(0.01)
$\triangle p_{t-1}$					0.028***	(0.01)
N	31,663		31,663		31,118	
Adj. R^2	0.581		0.583		0.585	

Notes: The dependent variable, $ln(Search)_t$, is the natural logarithm of the number of FuelCheck website visits on date t. Clustered standard errors at the SA2 level are reported in parentheses. ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels. All specifications control for *DISADV* dummies, SA2, weekend and month-of-year fixed effects.

Table 4.6 Disentangling cross-sectional and intertemporal search – Local SA2 Cycles

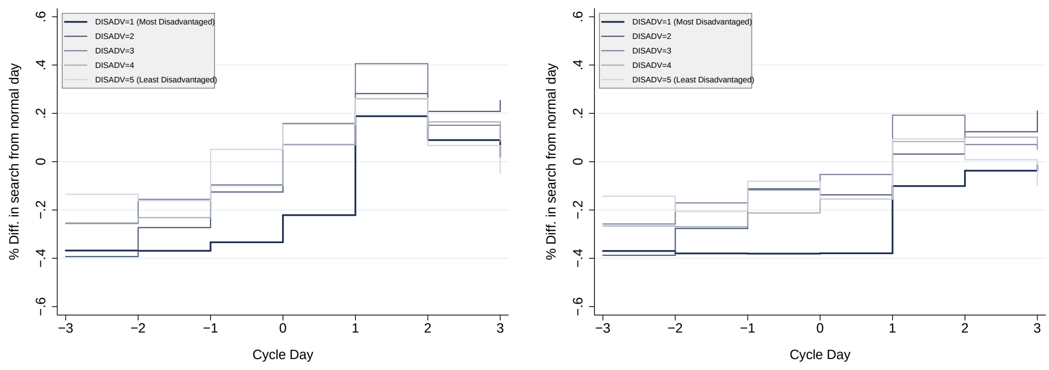
	(1)		(2)		(3)	
Base Category						
Day -3	-0.080*	(0.04)	-0.089**	(0.04)	-0.102**	(0.04)
Day -2	-0.069*	(0.04)	-0.065	(0.04)	-0.072*	(0.04)
Day -1	0.098**	(0.04)	0.095**	(0.04)	0.077	(0.06)
Day 0	0.232***	(0.04)	0.196***	(0.04)	0.163***	(0.06)
Day 1	0.236***	(0.04)	0.209***	(0.04)	0.256***	(0.06)
Day 2	0.029	(0.04)	0.018	(0.04)	0.026	(0.04)
Day 3	-0.016	(0.04)	-0.017	(0.04)	-0.019	(0.04)
σ_{p_t}			0.007***	(0.00)	0.006***	(0.00)
$\triangle p_t$					0.003	(0.00)
$\triangle p_{t+1}$					0.001	(0.00)
$\triangle p_{t-1}$					-0.005*	(0.00)
DISADV = 4						
Day -3	-0.123**	(0.06)	-0.112**	(0.06)	-0.108*	(0.06)
Day -2	-0.100	(0.06)	-0.110*	(0.06)	-0.118*	(0.06)
Day -1	-0.134**	(0.06)	-0.153**	(0.06)	-0.191**	(0.08)
Day 0	-0.066	(0.06)	-0.105*	(0.06)	-0.206**	(0.08)
Day 1	-0.052	(0.06)	-0.059	(0.06)	-0.164**	(0.08)
Day 2	0.140**	(0.06)	0.136**	(0.06)	0.100*	(0.06)
Day 3	-0.038	(0.06)	-0.045	(0.06)	-0.060	(0.06)
σ_{p_t}			0.011***	(0.00)	0.009***	(0.00)
$\triangle p_t$					0.009***	(0.00)
$\triangle p_{t+1}$					0.003	(0.00)
$\triangle p_{t-1}$					0.001	(0.00)
DISADV = 3						
Day -3	-0.074	(0.06)	-0.068	(0.06)	-0.064	(0.06)
Day -2	-0.084	(0.06)	-0.091	(0.06)	-0.096	(0.06)
Day -1	-0.108*	(0.06)	-0.108*	(0.06)	-0.167*	(0.09)
Day 0	0.035	(0.06)	0.071	(0.06)	-0.033	(0.09)
Day 1	0.109*	(0.06)	0.134**	(0.06)	0.055	(0.08)
Day 2	0.118**	(0.06)	0.127**	(0.06)	0.101	(0.06)
Day 3	0.055	(0.06)	0.073	(0.06)	0.064	(0.06)
σ_{p_t}			-0.001	(0.00)	-0.002	(0.00)

Continued on next page

Table 4.6 – continued from previous page

	(1)		(2)		(3)	
$\triangle p_t$					0.009***	(0.00)
$\triangle p_{t+1}$					0.005	(0.00)
$\triangle p_{t-1}$					-0.001	(0.00)
DISADV = 2						
Day -3	-0.202**	(0.09)	-0.197**	(0.09)	-0.176**	(0.09)
Day -2	-0.145*	(0.08)	-0.177**	(0.08)	-0.189**	(0.08)
Day -1	-0.160*	(0.09)	-0.194**	(0.09)	-0.265**	(0.12)
Day 0	-0.062	(0.08)	-0.063	(0.09)	-0.258**	(0.11)
Day 1	-0.005	(0.08)	-0.009	(0.09)	-0.260**	(0.12)
Day 2	0.090	(0.09)	0.014	(0.09)	-0.071	(0.10)
Day 3	0.034	(0.09)	0.020	(0.10)	-0.027	(0.10)
σ_{p_t}			0.007*	(0.00)	0.004	(0.00)
$\triangle p_t$					0.019***	(0.00)
$\triangle p_{t+1}$					0.006	(0.01)
$\triangle p_{t-1}$					0.013***	(0.01)
DISADV = 1						
Day -3	-0.245***	(0.07)	-0.235***	(0.07)	-0.220***	(0.07)
Day -2	-0.343***	(0.07)	-0.390***	(0.07)	-0.383***	(0.07)
Day -1	-0.372***	(0.07)	-0.389***	(0.07)	-0.356***	(0.09)
Day 0	-0.247***	(0.07)	-0.219***	(0.07)	-0.261***	(0.10)
Day 1	-0.177***	(0.07)	-0.152**	(0.07)	-0.338***	(0.10)
Day 2	0.064	(0.07)	0.087	(0.07)	0.034	(0.07)
Day 3	0.076	(0.07)	0.084	(0.07)	0.066	(0.07)
σ_{p_t}			0.000	(0.00)	-0.002	(0.00)
$\triangle p_t$					0.007*	(0.00)
$\triangle p_{t+1}$					-0.002	(0.00)
$\triangle p_{t-1}$					0.010***	(0.00)
N	31663		30634		27587	
Adj. R^2	0.574		0.582		0.583	

Notes: The dependent variable, $ln(Search)_t$, is the natural logarithm of the number of FuelCheck website visits on date t. Clustered standard errors at the SA2 level are reported in parentheses. ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels. All specifications control for *DISADV* dummies, SA2, weekend and month-of-year fixed effects.



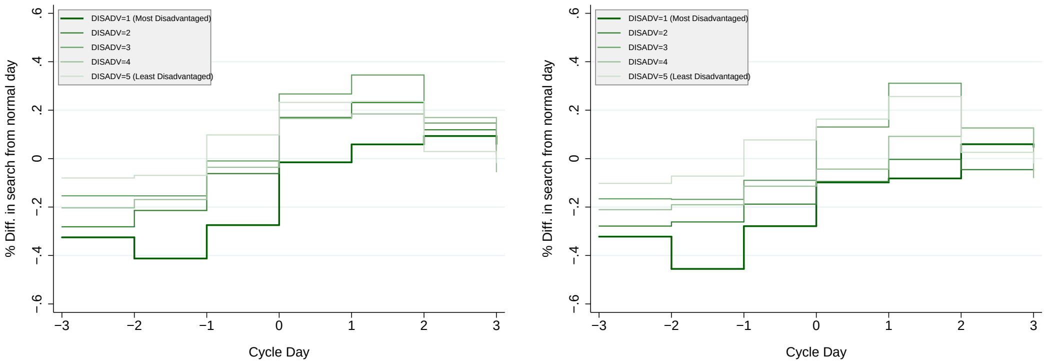
Notes: The figure on the left panel plots the baseline regression estimates and is a reproduction of Figure 4.7. The figure on the right panel plots the $\beta_1 + \gamma_k$ estimates from column (3) of Table 4.5 after including controls for cross-sectional and intertemporal price dispersion. They each represent the percentage change in average website visits on a given cycle day relative to a typical day in an IRSD group. Each line represents IRSD 1 (most disadvantaged), 2, 3, 4 and 5 (least disadvantaged) in increasing opacity.

Fig. 4.9 Search Response to Greater Sydney Cycles, “No Controls” vs “With Controls”

By comparing the left and right panels of Figures 4.9 and 4.10, it appears that the stair-step functions representing the changes the search intensity throughout a price cycle collapse after controlling for cross-sectional and intertemporal price dispersions.[33] This observation implies that search can be partially explained by changes in cross-sectional and intertemporal incentives in the market. The remaining variation in search intensities within each socio-economic group is likely explained by users using heuristics to decide when to search and react to price jumps above and beyond reacting to the magnitude of jumps or price dispersion around the events.

How do users from different socio-economic backgrounds react to cross-sectional or intertemporal search incentives in Greater Sydney? Referring to column (3) of Table 4.5, the results suggest that there is no relationship between search and price dispersion in the least disadvantaged socio-economic quintile and a negative and significant relationship between search and price dispersion in all the other socio-economic quintiles. Theoretically, a non-

[33] These are represented by the change in the magnitude of cycle day dummies in the regression after adding controls for cross-sectional and intertemporal price dispersion. The magnitudes are reported in Tables 4.5 and 4.6.



Notes: The figure on the left panel plots the baseline regression estimates and is a reproduction of Figure 4.8. The figure on the right panel plots the $\beta_1 + \gamma_k$ estimates from column (3) of Table 4.6 after including controls for cross-sectional and intertemporal price dispersion. They each represent the percentage change in average website visits on a given cycle day relative to a typical day in an IRSD group. Each line represents IRSD 1 (most disadvantaged), 2, 3, 4 and 5 (least disadvantaged) in increasing opacity.

Fig. 4.10 Search Response to Local SA2 Cycles, “No Controls” vs “With Controls”

monotone relationship between search and price dispersion can exist, which implies that the negative value of ϕ is still consistent with theory if the proportion of informed consumers is relatively high. However, it is unlikely that this is the case because my sample only includes data from the first few months after the website was launched and adoption rate is still very low. Therefore, this finding is unintuitive and likely pertains to an inappropriate market definition since defining the market at the city level implicitly assumes that consumers respond to price dispersion at over 700 gas stations. In retail gasoline markets, spatial aggregation matters and has the ability to influence results. For instance, Levin et al. (2017) show that by leveraging on the greater geographic detail of their data, they obtain more robust results and avoid biases potentially impacting the estimates and policy conclusions of previous studies employing aggregate data.

Therefore, I will focus my discussion on a more appropriate model that defines markets at the local SA2 neighbourhood level. Table 4.6 reports a positive relationship between search and price dispersion across all socio-economic groups. This relationship is more intuitive and aligned with the comparative statics reported in Chandra and Tappata (2011)’s paper.

The results show some heterogeneity in search reaction to cross-sectional and intertemporal price dispersion across socio-economic groups. First, it appears that while users from all socio-economic quintiles react positively to cross-sectional price dispersion, users from the 4th socio-economic quintile reacts to it the most. Second, users in the 1st, 2nd, 3rd and 4th socio-economic quintiles also react positively to intertemporal price changes, except for users in the least disadvantaged socio-economic quintile who barely react to intertemporal incentives.

To give the reader context, a one standard deviation increase in price dispersion, σ_{pt}, of 5.93 cpl on "day 0" increases search by 3.2% among users in the least disadvantaged socio-economic quintile. Similar magnitudes are found for users in the 1st, 2nd and 3rd socio-economic quintiles, with the exception of users in the 4th socio-economic quintile, who experience an increase in search by 8%. Likewise, a one standard deviation increase in the magnitude of the price jump on "day 0" does not significantly increase search for users in the least disadvantaged socio-economic quintile, increases search by 5.3% on the day itself for users in the 3rd and 4th socio-economic quintiles, increases search by 11% on the day itself for users in 2nd socio-economic quintile and increases search by 4% on the day itself for users in the lowest socio-economic quintile. Overall, it appears that users in the least disadvantaged socio-economic quintile only respond to contemporaneous deals, whereas the rest respond to both cross-sectional and intertemporal search incentives.

4.6 Conclusion

The violation of the Law of One Price has motivated many economists to study the relationship between search and price dispersion. Most, if not all, of these studies implicitly assume that the search-price dispersion relationships they establish are generalisable to all members of the population. Using unique data on search behaviour and retail prices across many retail gasoline markets, I find significant heterogeneities in the search-price dispersion relationship across socio-economic groups in the retail gasoline context.

This essay contributes to a relatively untapped field of analysing dynamic search using direct search measures. My analysis of search behaviour using high frequency data of a large cross-section of markets enable me to uncover new behavioural insights that are otherwise impossible with aggregate data. For instance, my results highlight new differences in search intensities across different socio-economic groups, particularly between the most disadvantaged users and all other users. Users in the lowest socio-economic group search the *least* and are least attentive to supply-side shocks (price jumps) relative to other users. In addition, my analysis also reveals some heterogeneity in search responses to cross-sectional and intertemporal price dispersion. Users from the highest socio-economic quintile appear to only respond to cross-sectional price dispersion, whereas users in the 1st, 2nd, 3rd and 4th socio-economic quintiles respond to both cross-sectional and intertemporal incentives.

A secondary contribution of my essay is in showing that spatial aggregation matters. Like Levin et al. (2017), I leverage on the high frequency and fine geographic resolution of my data and find that analysing data at different levels of market aggregations yield significantly different results. My initial results, obtained from a model that defines the retail gasoline market at the city level, are completely unintuitive and inaccurate compared to the results obtained from the disaggregated model. This highlights the importance of collecting data at finer resolutions whenever possible for future research into gasoline demand and retail price search.

Understanding how consumers search using online platforms such as FuelCheck, as well as the underlying mechanisms are important for policy. It is concerning that those who least effectively engage with FuelCheck are those who are the most vulnerable low-income users, and hence most impacted by supply side upward price shocks. Policies that help them time their purchases before price jumps could potentially lead to larger savings and fixes their commuting route by not having to deviate from their usual route to locate a cheaper station.

In light of my results, I suggest implementing a "paternalistic libertarian" approach of indirectly influencing behaviour in the spirit of Thaler and Sunstein (2009) as a cost-effective and unobtrusive method of improving user engagement on search platforms such as FuelCheck. Nudge interventions have been successfully employed in many policy contexts where individuals make suboptimal or irrational decisions such as organ donation, energy conservation, retirement savings and education and are increasingly used in conjunction with conventional policies.[34]

Being able to separate searches that appear to be best characterised as myopic is useful for setting policy setting. For users who appear to demonstrate myopic behaviour, the proposed intervention should effectively change their present bias behaviour to more forward-looking behaviour. A possible solution is to send out reminders to platform users to the effect of notifying them of upcoming price jumps in their local area or usual commuting route. Doing so helps users predict price jumps and overcomes the need to process large amounts of information that a lot of lower socio-economic users find challenging. Sending notifications directly to users provide an accessible means of obtaining price information in the market, and has previous shown to be a more effective way of overcoming comparison frictions[35] than say, actively seeking buying tips on the competition authority's website.

[34]Here are a few examples of behavioural interventions that have been successfully trialled or implemented. In one of the largest randomised controlled trials ever run in the UK, the behavioural Insights Team found that sending different message variants to encourage people to join the NHS Organ Donor Register result in different sign up rates, indicating that message design matters Behavioural Insights Team (2013). Another study by Johnson and Goldstein (2003) find that countries with a no-action default policy for organ donation report higher rates of donations than countries with an opt-in policy. In addition, Beshears et al. (2013) find that simplifying the enrollment process into employer-sponsored savings plans, individual enrollments increase.

[35]For instance, Hastings and Weinstein (2008) and Kling et al. (2012) find that individuals who are provided information on school quality and prescription drug plans make better school choice and increases switching rate of prescription drug plans as opposed to individuals who are required to actively seek the same information.

Chapter 5

Conclusion

This thesis uses empirical methods to explore the interplay of search frictions and market power within economics. Overall, the three chapters provide new avenues for future research. Chapter 2 contributes to the literature on search frictions and price dispersion by examining search frictions and price negotiation as sources of price dispersion and market power. We find that in the context of retail electricity, firms are willing to lower profit margins for consumers who reveal a low reference price from a competitor firm. In addition, we also find that consumers who reveal that they are an existing consumer of a rival firm are offered lower prices than consumers who are new to the market. Our use of the audit study approach to disentangle search frictions and price negotiation as sources of price dispersion is innovative and highlights the value of using this approach for studying oligopoly problems more generally.

The third and fourth chapters contribute to our understanding of how price transparency platforms affect firm competition and consumer search in the retail gasoline context. Chapter 3 examines the impact of a mandatory price disclosure policy on market competition. My low frequency margin analysis shows that the policy has led to margin-enhancing effects in small regional markets. Then, using real-time station-level prices, I show that the margin-enhancing effects can be explained by a coordinated transition to a more profitable pricing equilibrium.

These evidences suggest that firms were potentially using the information disclosure platform to tacitly coordinate with their rivals. As the literature on how firms tacitly collude is still young, there remains vast opportunities for developing theories that try to understand this phenomenon.

Chapter 4 examines the impact of supply-side price shocks on consumer search across socio-economic groups. I find that amongst all users of the price disclosure platform, users from the most disadvantaged socio-economic group search the least and are slowest to react to supply-side price shocks. In this way, my findings are revealing the importance of other constraints disadvantaged groups face, such as lower cognitive skills and rational inattention that limit their ability to search. Therefore, policies that help vulnerable consumers overcome challenging intertemporal search could be beneficial.

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Appendix A

Appendices of Chapter 2

A.1 Bargaining Scripts

Samples of the bargaining scripts used in our field experiment are attached here. Each script attached here corresponds to a variation in information source from which the reference price is obtained: 1) Called other retailer(s), 2) Government comparator website and 3) Friend. These scripts are relevant for callers who called as fictitious new consumers in the market.

Bargaining Script C (Called Around)

You are now about to call **retailer X**. Please note that you have a 20-minute time limit to complete this call. If you run out of time, please conclude the call.

Introduce yourself based on your role description. Please bear in mind that they may ask more questions than the ones on this script and to answer them you should refer to Document A. ***Important: Not all questions will be asked of you. Please do not provide answers not asked of you unless prompted on the script.***

SECTION 1: DO NOT REVEAL SEARCH METHOD

Introduction

RETAILER: Hi, you are calling **retailer X**. My name is (sales agent's name). How can I help you?

YOU: Hi. I want to have electricity connected to my new place. I am moving from interstate. What are your rates?

I'm also eligible for an Energy Concession (*if applicable, else say nothing unless asked)*

Only if asked:
- I'm looking for a one year contract
- We use about 10kWh per day or 300kWh per month

(Note: They may ask if you're interested in signing up online, in which case just say you haven't decided but ask if there's a discount for that. Also ask if discount can be applied over the phone.)

Address

RETAILER: Sure, may I have your address or NMI please?

YOU: We will be moving to **address**.

Only if asked:

RETAILER:	Do you have solar panels at your new property?	YOU: No
RETAILER:	Is there a pool at your new property?	YOU: No
RETAILER:	Are you interested in gas as well?	YOU: No
RETAILER:	Green energy?	YOU: No

First Price Quote

RETAILER: OK. We can offer you our **(name of electricity plan)**. It's **XXXXX** cents/kWh along with a **XXXXX** cents/day supply charge.

YOU: Does that include GST?

RETAILER:	No, that is ex-GST.
YOU:	Is there a discount for direct debit?
RETAILER:	That plan already includes a discount for direct debit.
YOU:	How much would I have to pay without direct debit?
RETAILER:	**XXXXX**% more
YOU:	So both the supply and variable charges would be that much more?
RETAILER:	**Yes/No**.
YOU:	*Confirm all of the following:* • 12 month contract • Monthly bills • Bills sent via email • No/any penalty or exit fee if I end the contract early?
YOU:	Does that price only apply if I pay my bill on time? If I don't, how much would I need to pay?
RETAILER:	**Yes/No.** If you don't pay on time, your total bill will be **XXXX**% higher.
YOU:	Does my rate increase at the end of my contract?
RETAILER:	No. Rates only increase with inflation and when we have to pass on annual increases in network charges.

SECTION 2: REVEAL PRIOR SEARCH METHOD

	Second Price Quote
YOU:	Is this your best price? I have called **one company/a few other companies** before you and I have been offered a better deal.
RETAILER:	Which company is offering you this price?
YOU:	**XXXXXXXX**
RETAILER:	And what did they offer?
YOU:	**XXXXXX** per day and **XXXXXX** per kWh.
RETAILER:	*(Retailer either provides lower new price or refuses to lower price)*
YOU:	Is this new plan also valid for 12 months? Are there any penalty fees?
YOU:	Can I also ask if there is a discount for direct debit payment? This is a pay-on-time price, right? How much would I pay without direct debit/without pay-on-time? *(record any price revisions and new plan details)*

SECTION 3: BARGAIN EVEN FURTHER

Final Price Quote	
YOU:	Is that really the lowest you can go? I was hoping to get a better price because a **friend** of mine told me he pays less than **$850** a year for electricity.
RETAILER:	What company does your friend use?
YOU:	I don't know.
RETAILER:	Do you know his supply and variable charges?
YOU:	No.
	(please give retailer time to respond and record any price revisions and new plan details)

Ending the Conversation	
YOU:	Thank you for your help today. I'll talk with my partner and get back to you. Have a good day. Bye.
	You should decline any offer to call you back. They are likely to insist, and you should just end the conversation by asking for an ID number that you can quote if you decide to call back later followed by "Thank you for your help today. I'll talk with my partner and get back to you. Have a good day. Bye."

--End of Conversation—

Important:

All conversations must be kept to a maximum of **20 minutes**. The research assistant sitting beside you will notify you at the 15th-minute and 19th-minute mark. At the **second prompt, <u>you should wrap up the conversation</u>**. If you do not manage to complete all the stages of the bargaining script, kindly inform the researcher in-charge.

In the event that this happens, you may end the conversation by saying "Sorry, I'm afraid I need to go now. I have an appointment in a few minutes. Thank you for your help. Bye."

Bargaining Script W (Website)

You are now about to call retailer X. Please note that you have a 20-minute time limit to complete this call. If you run out of time, please conclude the call using the method displayed at the end of this script.

Introduce yourself based on your role description. Please bear in mind that they may ask more questions than the ones on this script and to answer them you should refer to Document A. ***Important: Not all questions will be asked of you. Please do not provide answers not asked of you unless prompted on the script.***

SECTION 1: DO NOT REVEAL SEARCH METHOD

Introduction

RETAILER: Hi, you are calling **retailer X**. My name is (sales agent's name). How can I help you?

YOU: Hi. I want to have electricity connected to my new place. I am moving from interstate. What are your rates?

I'm also eligible for an Energy Concession (*if applicable, else say nothing unless asked)*

Only if asked:

- I'm looking for a one year contract
- We use about 10kWh per day or 300kWh per month

(Note: They may ask if you're interested in signing up online, in which case just say you haven't decided but ask if there's a discount for that. Also ask if discount can be applied over the phone.)

Address

RETAILER: Sure, may I have your address or NMI please?

YOU: We will be moving to **address.**

Only if asked:

RETAILER:	Do you have solar panels at your new property?	YOU: No
RETAILER:	Is there a pool at your new property?	YOU: No
RETAILER:	Are you interested in gas as well?	YOU: No
RETAILER:	Green energy?	YOU: No

First Price Quote

RETAILER: OK. We can offer you our **(name of electricity plan)**. It's XXXXX cents/kWh along with a XXXXX cents/day supply charge.

YOU: Does that include GST?

RETAILER:	No, that is ex-GST.
YOU:	Is there a discount for direct debit?
RETAILER:	That plan already includes a discount for direct debit.
YOU:	How much would I have to pay without direct debit?
RETAILER:	XXXXX% more
YOU:	So both the supply and variable charges would be that much more?
RETAILER:	Yes/No.
YOU:	*Confirm all of the following:* • 12 month contract • Monthly bills • Bills sent via email • No/any penalty or exit fee if I end the contract early?
YOU:	Does that price only apply if I pay my bill on time? If I don't, how much would I need to pay?
RETAILER:	Yes/No. If you don't pay on time, your total bill will be XXXX% higher.
YOU:	Does my rate increase at the end of my contract?
RETAILER:	No. Rates only increase with inflation and when we have to pass on annual increases in network charges.

SECTION 2: REVEAL SEARCH METHOD

	Second Price Quote
YOU:	Is this your best price? I checked out the **comparison website "SwitchOn"** today and found that **Globird** offers the best deal for my usage level at **$970** a year.
RETAILER:	What are the plan details?
YOU:	**78.4** cents per day and **19.24** cents per kWh
RETAILER:	*(Retailer either provides lower new price or refuses to lower price)*
YOU:	Is this new plan also valid for 12 months? Are there any penalty fees?
YOU:	Can I also ask if there is a discount for direct debit payment? This is a pay-on-time price, right? How much would I pay without direct debit/without pay-on-time? *(record any price revisions and new plan details)*

SECTION 3: BARGAIN EVEN FURTHER

Final Price Quote	
YOU:	Is that really the lowest you can go? I was hoping to get a better price because a **friend** of mine told me he pays less than **$850** a year for electricity.
RETAILER:	What company does your friend use?
YOU:	I don't know.
RETAILER:	Do you know his supply and variable charges?
YOU:	No.
	(please give retailer time to respond and record any price revisions and new plan details)

Ending the Conversation	
YOU:	Thank you for your help today. I'll talk with my partner and get back to you. Have a good day. Bye. *You should decline any offer to call you back. They are likely to insist, and you should just end the conversation by asking for an ID number that you can quote if you decide to call back later followed by* "Thank you for your help today. I'll talk with my partner and get back to you. Have a good day. Bye."

--End of Conversation—

Important:

All conversations must be kept to a maximum of **20 minutes**. The research assistant sitting beside you will notify you at the 15th-minute and 19th-minute mark. At the **second prompt, <u>you should wrap up the conversation</u>**. If you do not manage to complete all the stages of the bargaining script, kindly inform the researcher in-charge.

In the event that this happens, you may end the conversation by saying "Sorry, I'm afraid I need to go now. I have an appointment in a few minutes. Thank you for your help. Bye."

Bargaining Script F (Friend)

You are now about to call **retailer X**. Please note that you have a 20-minute time limit to complete this call. If you run out of time, please conclude the call using the method displayed at the end of this script.

Introduce yourself based on your role description. Please bear in mind that they may ask more questions than the ones on this script and to answer them you should refer to Document A. ***Important: Not all questions will be asked of you. Please do not provide answers not asked of you unless prompted on the script.***

SECTION 1: DO NOT REVEAL SEARCH METHOD

Introduction

RETAILER: Hi, you are calling **retailer X**. My name is (sales agent's name). How can I help you?

YOU: Hi. I want to have electricity connected to my new place. I am moving from interstate. What are your rates?

I'm also eligible for an Energy Concession (*if applicable, else say nothing unless asked)*

Only if asked:

- I'm looking for a one year contract
- We use about 10kWh per day or 300kWh per month

(Note: They may ask if you're interested in signing up online, in which case just say you haven't decided but ask if there's a discount for that. Also ask if discount can be applied over the phone.)

Address

RETAILER: Sure, may I have your address or NMI please?

YOU: We will be moving to **address.**

Only if asked:

RETAILER:	Do you have solar panels at your new property?	YOU: No
RETAILER:	Is there a pool at your new property?	YOU: No
RETAILER:	Are you interested in gas as well?	YOU: No
RETAILER:	Green energy?	YOU: No

First Price Quote

RETAILER: OK. We can offer you our **(name of electricity plan)**. It's **XXXXX** cents/kWh along with a **XXXXX** cents/day supply charge.

YOU: Does that include GST?

RETAILER:	No, that is ex-GST.
YOU:	Is there a discount for direct debit?
RETAILER:	That plan already includes a discount for direct debit.
YOU:	How much would I have to pay without direct debit?
RETAILER:	**XXXXX**% more
YOU:	So both the supply and variable charges would be that much more?
RETAILER:	**Yes/No**.
YOU:	*Confirm all of the following:* • 12 month contract • Monthly bills • Bills sent via email • No/any penalty or exit fee if I end the contract early?
YOU:	Does that price only apply if I pay my bill on time? If I don't, how much would I need to pay?
RETAILER:	**Yes/No.** If you don't pay on time, your total bill will be **XXXX**% higher.
YOU:	Does my rate increase at the end of my contract?
RETAILER:	No. Rates only increase with inflation and when we have to pass on annual increases in network charges.

SECTION 2: REVEAL PRIOR SEARCH METHOD

	Second Price Quote
YOU:	Is this your best price? I was hoping to get a better price because a **friend** of mine told me he pays **XXXXXX** per day and **XXXXXX** per kWh.
RETAILER:	What company does your friend use?
YOU:	**Globird**
RETAILER:	*(Retailer either provides lower new price or refuses to lower price)*
YOU:	Is this new plan also valid for 12 months? Are there any penalty fees?
YOU:	Can I also ask if there is a discount for direct debit payment? This is a pay-on-time price, right? How much would I pay without direct debit/without pay-on-time? *(record any price revisions and new plan details)*

SECTION 3: THREATEN TO SEARCH IN THE FUTURE

Final Price Quote	
YOU:	Is that the absolute lowest you can go? *(please wait for retailer to respond)*
YOU:	*(If yes)* If I can't get a better rate then I'll need to keep searching for another better deal. *(please give retailer time to respond and record any price revisions and new plan details)*

Ending the Conversation	
YOU:	Thank you for your help today. I'll talk with my partner and get back to you. Have a good day. Bye. *You should decline any offer to call you back. They are likely to insist, and you should just end the conversation by asking for an ID number that you can quote if you decide to call back later followed by* "Thank you for your help today. I'll talk with my partner and get back to you. Have a good day. Bye."

--End of Conversation—

Important:

All conversations must be kept to a maximum of **20 minutes**. The research assistant sitting beside you will notify you at the 15th-minute and 19th-minute mark. At the **second prompt, <u>you should wrap up the conversation</u>**. If you do not manage to complete all the stages of the bargaining script, kindly inform the researcher in-charge.

In the event that this happens, you may end the conversation by saying "Sorry, I'm afraid I need to go now. I have an appointment in a few minutes. Thank you for your help. Bye."

A.2 Price Sheet

Attached here is a standardised price sheet used by our callers in the experiment. Callers were only asked to update price information in subsequent stages of a call if a price revision was made in that stage.

Price Sheet

Name: [illegible] Date: 5/4/17

Company Name: [illegible] (e.g. Origin)

Please fill in this section before making the phone call:

Variation ID:	F2	**Search Method:**	
Address:	[illegible]	Called One / Called Many / Website / Friend	
Concession:	Eligible / Not Eligible	**Price-to-Beat:** 19.24 Usage	78.4 Supply; $970 Annual (300kWh/mo)

Price #1 – price obtained after revealing address and/or concession

Non-direct debit offer	Direct debit offer
1. Usage charge (cents **per kWh**, incl. GST): 21.49 INC GST + 27%	**1. Usage charge:** **Direct Debit discount:** 3%
Pay-on-time/Not pay-on-time 27%	Pay-on-time/Not pay-on-time 30%
2. Supply charge (cents **per day**, incl. GST): 82.14 INC GST + 27%	**2. Supply charge:**
Pay-on-time/Not pay-on-time 27%	Pay-on-time/Not pay-on-time 30%
3. Rate if pay-on-time/not pay-on-time:	**3. Rate if pay-on-time/not pay-on-time:**

Other Discounts and offers (e.g. One-off rebates, movie tickets):

Exit Fee:

How long are discounts valid for?

ONGOING

Please ensure that the call only proceeds to the next stage after you have acquired the details for the current stage.

Price #2 – price obtained after revealing search method

Non-direct debit offer	Direct debit offer	
1. Usage charge (cents **per kWh**, incl. GST):	**1. Usage charge:**	**Direct Debit discount:**
Pay-on-time/Not pay-on-time	Pay-on-time/Not pay-on-time	
2. Supply charge (cents **per day**, incl. GST):	**2. Supply charge:**	
Pay-on-time/Not pay-on-time	Pay-on-time/Not pay-on-time	
3. Rate if pay-on-time/not pay-on-time:	**3. Rate if pay-on-time/not pay-on-time:**	

Other Discounts and offers (e.g. One-off rebates, movie tickets):

Exit Fee:

How long are discounts valid for?

Price #3 – price obtained after friend's very low price or threatening to search more

Non-direct debit offer	Direct debit offer	Direct Debit discount:
1. Usage charge (cents **per kWh**, incl. GST):	**1. Variable charge:**	
Pay-on-time/Not pay-on-time	Pay-on-time/Not pay-on-time	
2. Supply charge (cents **per day**, incl. GST):	**2. Supply charge:**	
Pay-on-time/Not pay-on-time	Pay-on-time/Not pay-on-time	
3. Rate if pay-on-time/not pay-on-time:	**3. Rate if pay-on-time/not pay-on-time:**	

Other Discounts and offers (e.g. One-off rebates, movie tickets):

$50 CREDIT OFF FIRST BILL

Exit Fee:

How long are discounts valid for?

Other Notes:

Appendix B

Appendices for Chapter 3

Table B.1 Effects of disclosure on margins (excludes Coles' termination from Informed Sources' service)

	(1)	(2)	(3)
FuelCheck Disclosure	2.226	2.226	3.751
p-values	[0.0550]	[0.0550]	[0.0498]
State FE	Yes	Yes	Yes
Time Trend	Yes	Yes	Yes
City FE	No	Yes	No
City X Month-of-Year FE	No	No	Yes
Month-of-Year FE	No	Yes	No
N	1581	1581	1581
Average margin	15.28	15.28	15.28
Effect as % of the mean	14.56%	14.56%	24.5%
Adj. R^2	0.225	0.617	0.537

Notes: The *p*-values associated with the 6-point distribution Wild Bootstrapping procedure using Webb (2014) weights and 9,999 replications are reported in square brackets. Bootstraps are clustered at the state level, which is the level of policy variation. The dependent variable for all specifications is the inflation-adjusted margins (in Australian cents per litre), computed as the difference between average city-level retail price and wholesale TGP in month-year t (e.g. January 2017). The sample includes 93 cities and the sample period extends from October 2015 to June 2017, excluding April 2016 to July 2016.

Table B.2 Market Structure

City	Pop. Quin.	No. of Stations, by Brand					
		BP	Caltex	Coles	Woolworths	7-Eleven	Independent
Bega	1	0	2	1	1	0	0
Hay	1	0	2	0	0	0	0
Lismore	1	2	2	2	0	0	11
Narrabri	1	1	0	0	0	0	3
Oberon	1	0	0	0	0	0	3
Yass	1	0	0	0	0	0	1
Casino	2	0	2	1	0	0	8
Cooma	2	0	1	0	1	0	5
Forbes	2	1	1	0	1	0	3
Inverell	2	0	2	1	1	0	4
Kempsey	2	1	3	1	1	0	3
Moree	2	2	1	1	0	0	0
Broken Hill	3	0	1	1	1	0	5
Forster-Tuncurry	3	0	1	1	0	0	3
Grafton	3	2	4	1	1	0	3
Griffith	3	1	0	1	1	0	3
Taree	3	1	1	1	1	0	3
Ulladulla	3	1	0	0	0	0	1
Albury	4	3	3	2	2	0	5
Bathurst	4	0	2	1	0	2	3
Coffs Harbour	4	2	3	1	1	0	7
Dubbo	4	0	2	2	1	0	5
Goulburn	4	0	3	2	2	1	4
Orange	4	0	3	1	1	1	4
Port Macquarie	4	1	2	1	1	0	2
Tamworth	4	2	2	2	0	0	5
Wagga Wagga	4	1	3	1	1	0	15
Newcastle	5	13	9	5	5	15	22
Sydney	5	62	105	58	47	126	216
Wollongong	5	5	13	2	4	8	21

Notes: This table tabulates tabulates the number of gas stations for each of the cities in the state of New South Wales in the analysis. The first four brands – BP, Caltex, Coles and Woolworths – correspond to C4 brands. The remaining stations are categorised as independent stations.

High Frequency Plots of Station-Level Price Updates

This section contains the high frequency plots of station-level Price Updates on FuelCheck for New South Wales' cities in my sample. Each observation corresponds to a station-level price update. The number of legend labels correspond to the total number of stations in the city, and are distinguished by station brands.

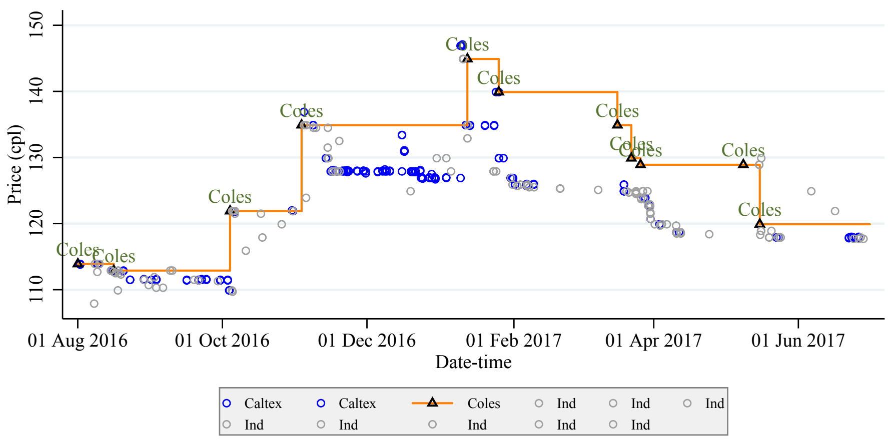

Fig. B.1 Station-level Price Updates – Casino

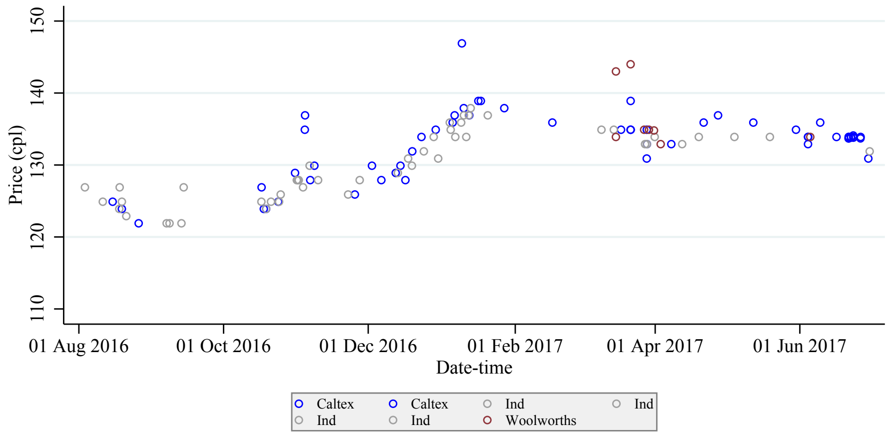

Fig. B.2 Station-level Price Updates – Cooma

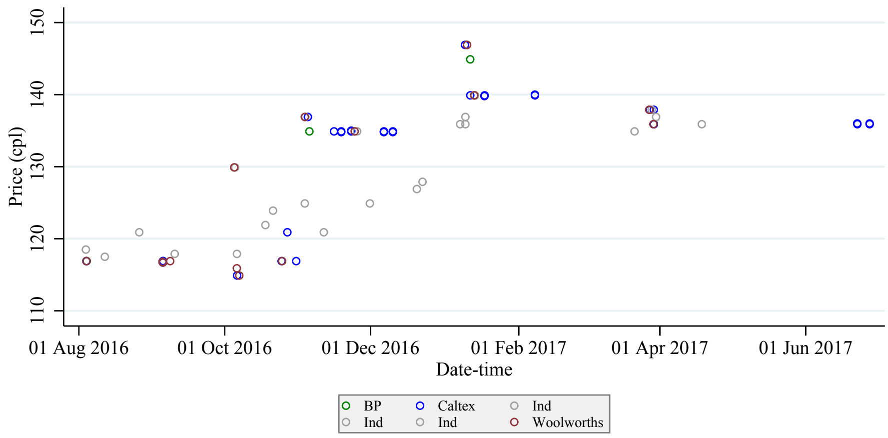

Fig. B.3 Station-level Price Updates – Forbes

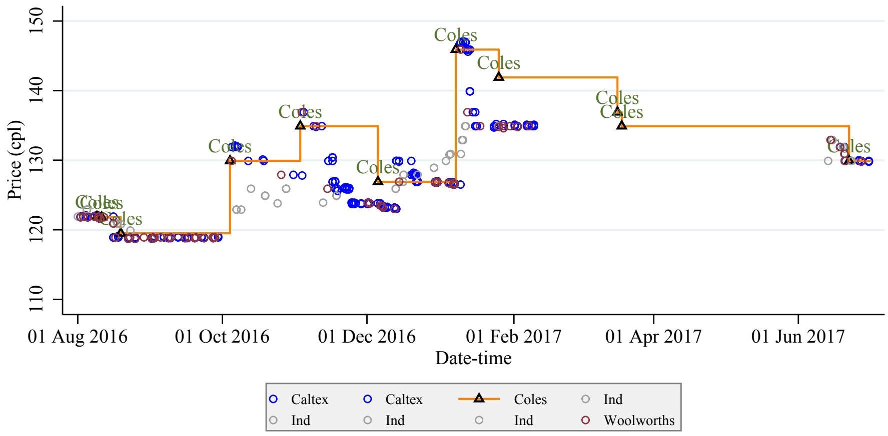

Fig. B.4 Station-level Price Updates – Inverell

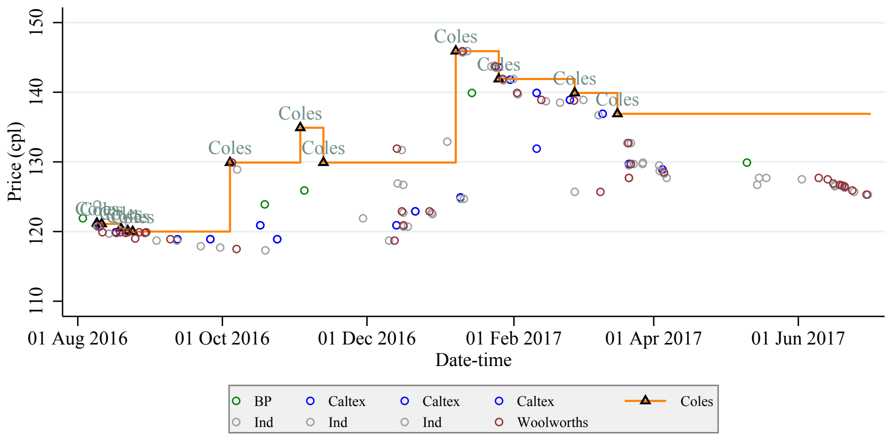

Fig. B.5 Station-level Price Updates – Kempsey

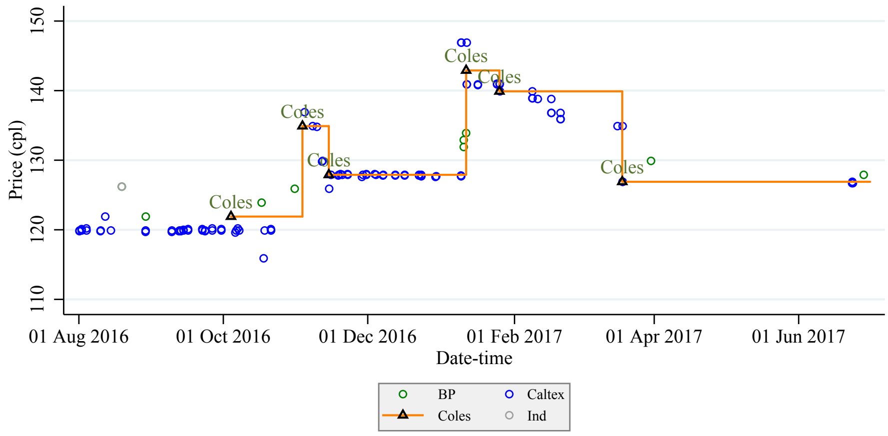

Fig. B.6 Station-level Price Updates – Moree

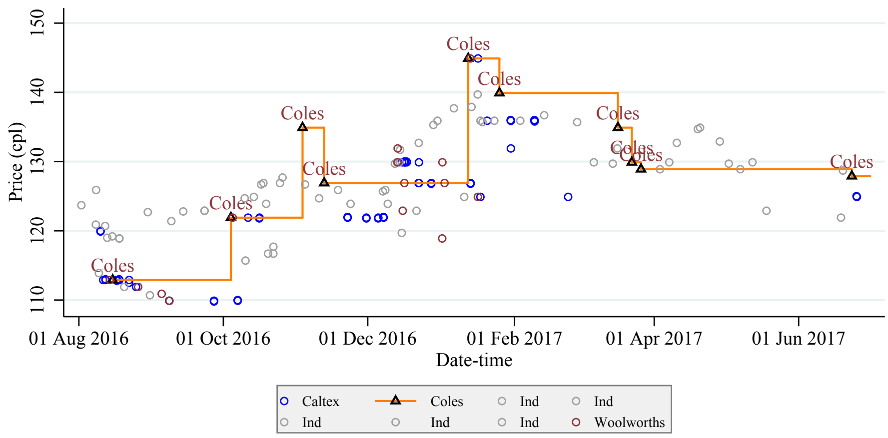

Fig. B.7 Station-level Price Updates – Broken Hill

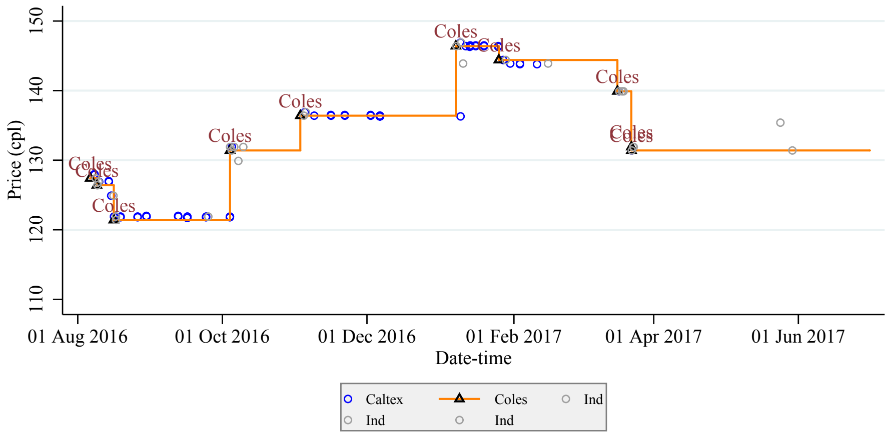

Fig. B.8 Station-level Price Updates – Forster-Tuncurry

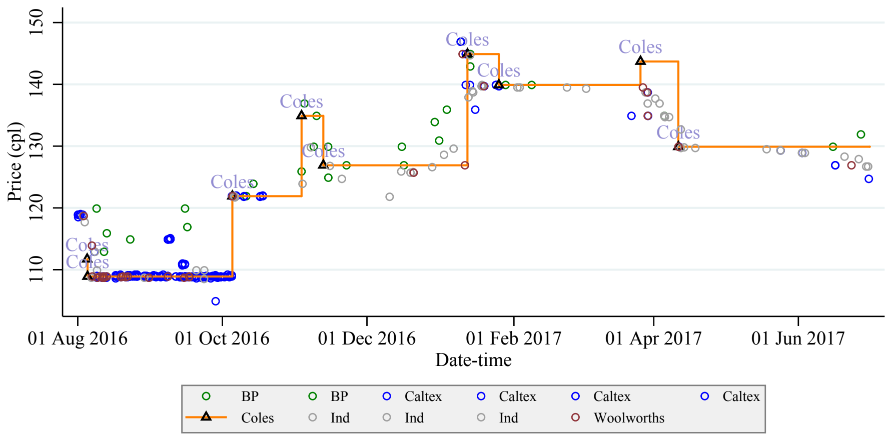

Fig. B.9 Station-level Price Updates – Grafton

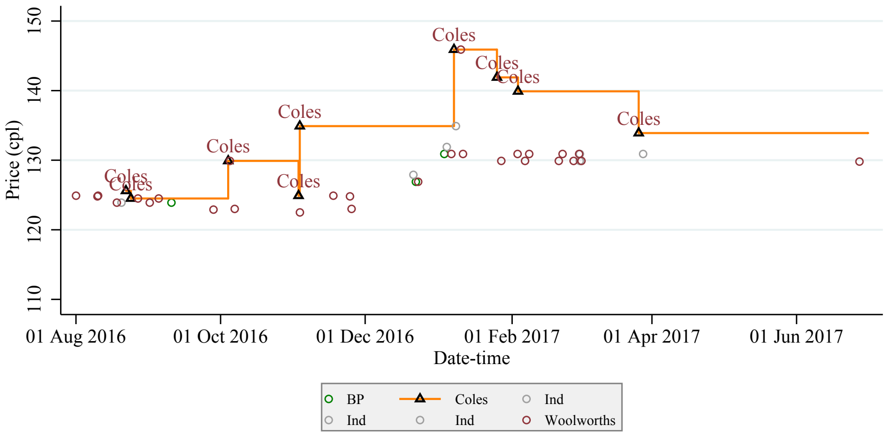

Fig. B.10 Station-level Price Updates – Griffith

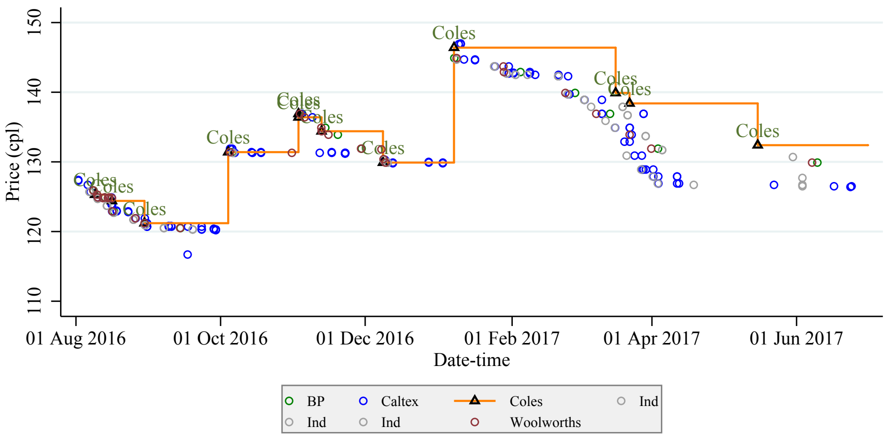

Fig. B.11 Station-level Price Updates – Taree

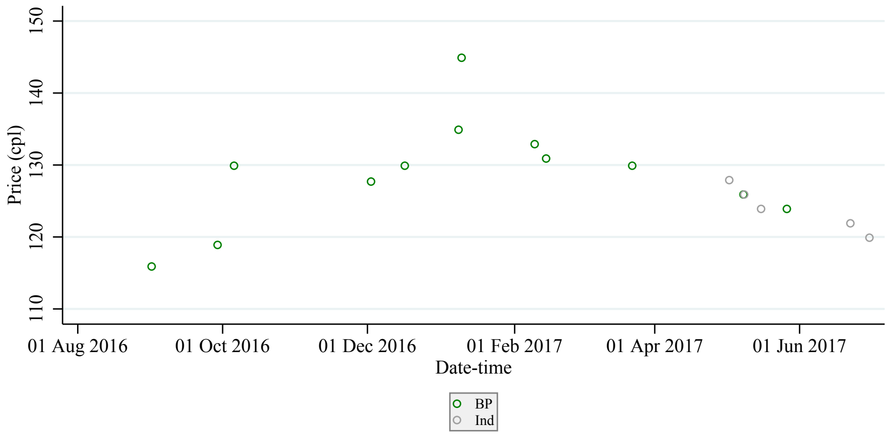

Fig. B.12 Station-level Price Updates – Ulladulla

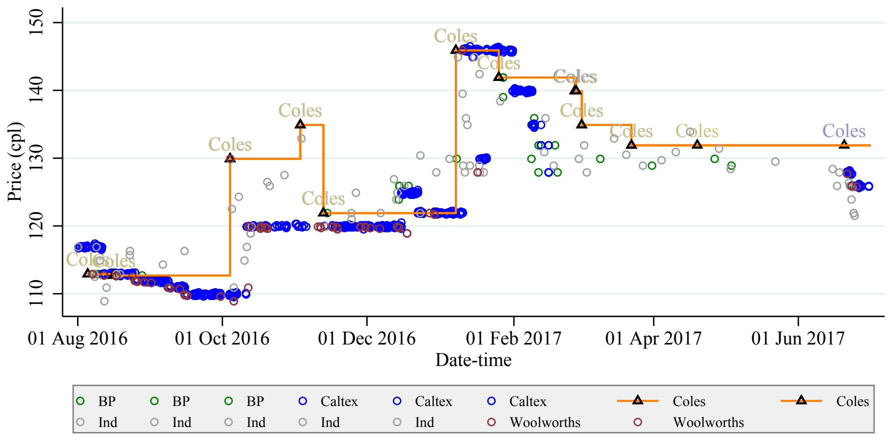

Fig. B.13 Station-level Price Updates – Albury

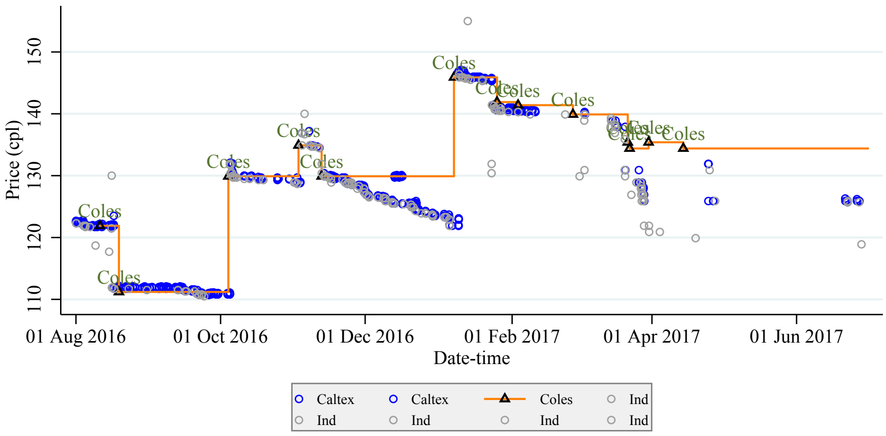

Fig. B.14 Station-level Price Updates – Bathurst

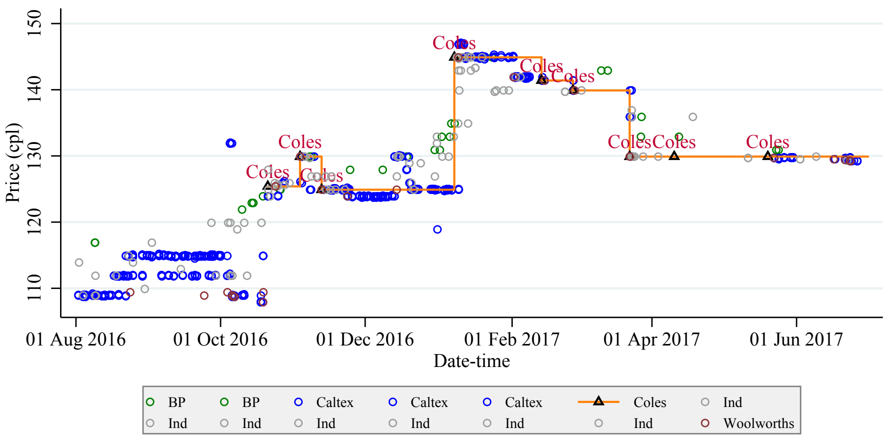

Fig. B.15 Station-level Price Updates – Coffs Harbour

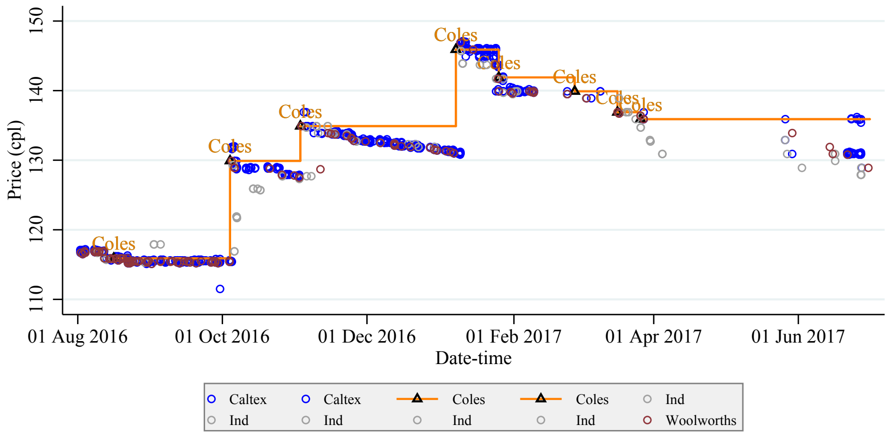

Fig. B.16 Station-level Price Updates – Dubbo

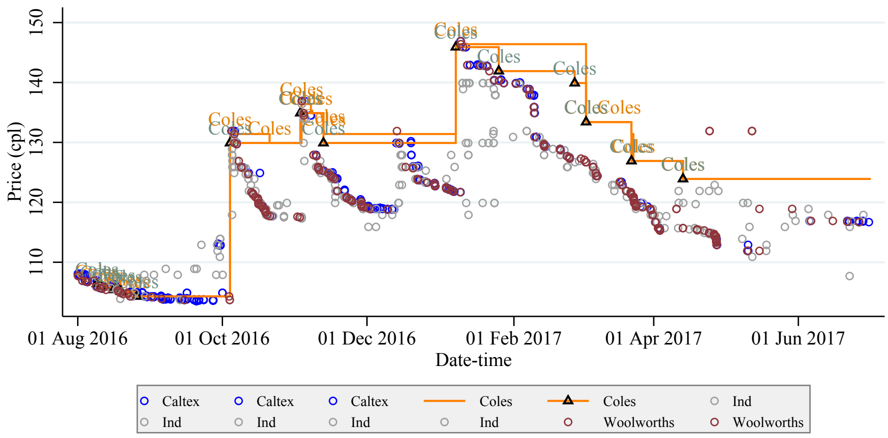

Fig. B.17 Station-level Price Updates – Goulburn

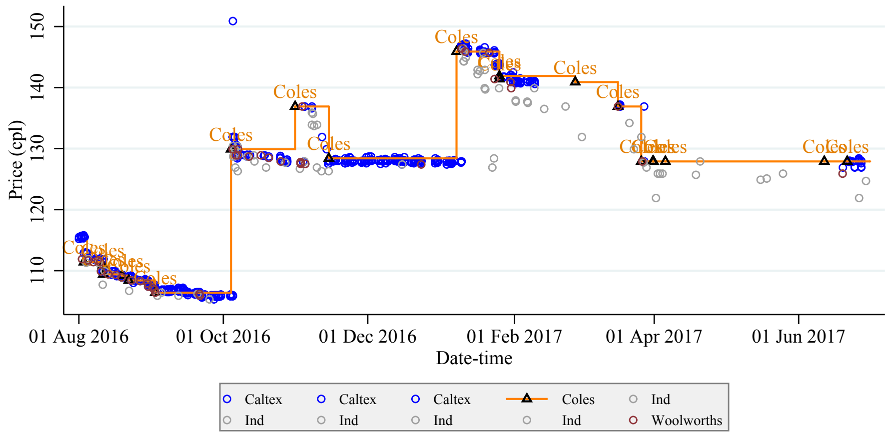

Fig. B.18 Station-level Price Updates – Orange

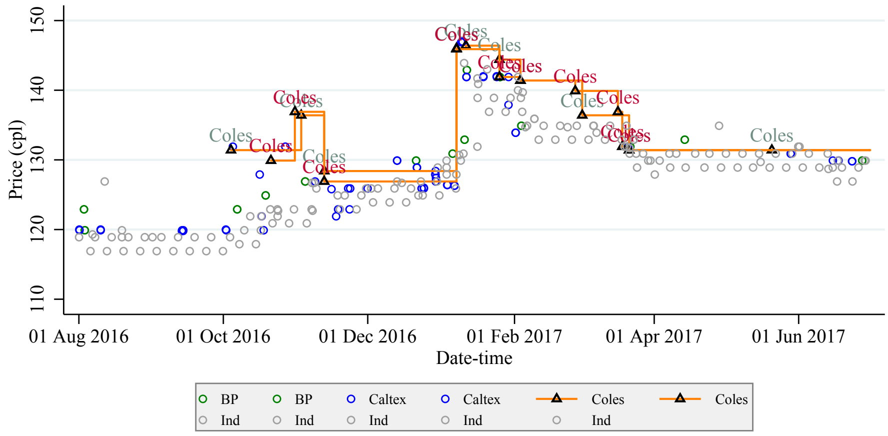

Fig. B.19 Station-level Price Updates – Tamworth

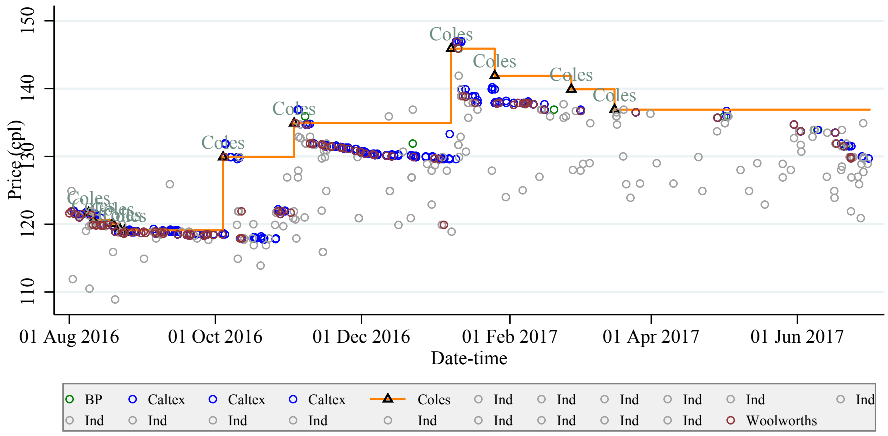

Fig. B.20 Station-level Price Updates – Wagga Wagga

Appendix C

Appendices of Chapter 4

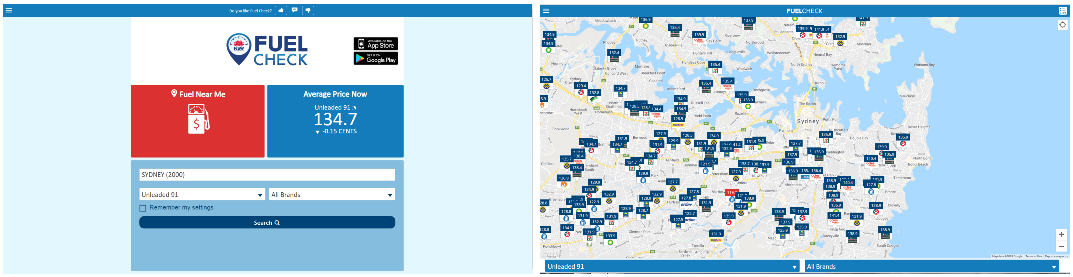

Fig. C.1 Display of Fuelcheck's Website

Table C.1 Search Responses to Sydney Cycle Days (Robustness Analysis, SA2 and Fixed Radius Market Definitions)

	SA2 Market	**Fixed Radius**		
		1km	3km	5km
Day -3	-0.261***	-0.260***	-0.260***	-0.268***
	(0.021)	(0.013)	(0.009)	(0.008)
Day -2	-0.230***	-0.200***	-0.236***	-0.237***
	(0.019)	(0.012)	(0.008)	(0.007)
Day -1	-0.109***	-0.088***	-0.093***	-0.100***
	(0.022)	(0.012)	(0.009)	(0.008)
Day 0	0.052**	0.09***	0.068***	0.058***
	(0.021)	(0.012)	(0.009)	(0.008)
Day 1	0.279***	0.268***	0.267***	0.262***
	(0.019)	(0.011)	(0.008)	(0.007)
Day 2	0.128***	0.154***	0.144***	0.145***
	(0.020)	(0.012)	(0.009)	(0.007)
Day 3	0.060***	0.046***	0.072***	0.08***
	(0.021)	(0.012)	(0.008)	(0.007)
N	31663	120,457	129,699	131,014
Adj. R^2	0.577	0.559	0.824	0.885

Notes: The dependent variable, $ln(Search)_t$, is the natural logarithm of the number of FuelCheck website visits on date t. Clustered standard errors at the SA2 or station level are reported in parentheses. ***, ** and * indicate statistical significance at the 1%, 5% and 10% levels. All specifications control for SA2 or station, weekend and month-of-year fixed effects.